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Modern Radiology
In
Historical Perspective

Modern Radiology In Historical Perspective

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FOREWORD

Most radiologic trainees do not supplement their academic and hospital training with non-required study.

Accordingly, because I believed that every radiologic trainee should have the opportunity to receive more than the basic requirements, I programmed into our 1958 radiologic curriculum a series of lectures on the cultural and historical aspects of radiology in order to provide a broader intellectual background.

After thorough deliberation and with full confidence, I assigned Dr. Stephen B. Dewing to this task.

These lectures were an instant success, for they supplied significance in perspective to the specialty, thereby endowing it with overtones of prestige and heritage.

Now these lectures have been combined into this book, which is indeed further eloquent evidence of Dr. Dewing's valuable contribution to continuing education in the cultural and historical aspects of radiology.

MAXWELL H. POPPEL, M.D.

PREFACE

THIS BOOK HAD its origin in a series of historical lectures presented first during the academic year 1958-1959 to the doctors pursuing post-graduate training in radiology at Bellevue Hospital in New York City. The impetus to prepare them came from my academic chief, Dr. M. H. Poppel, Professor and Chairman of the Radiology Department at the College of Medicine of New York University.

When Dr. Poppel suggested the project in August 1958, I was at once interested and intrigued. Without underrating the fact that there would be some real work involved I blithely assumed that I already had pretty good control of the subject. It took me most of the ensuing year, spending every available moment on research, to keep one jump ahead of the lecture schedule. The volume of historical material was simply tremendous, and I gradually discovered how very much I did *not* know. Caught up in the busy pace of post-graduate training I had become so preoccupied with the practical business of learning and applying currently useful facts and figures that I had taken the background for granted. It may be that there are others too in the same position.

My object has been first, to provide a historical perspective and second, to sketch the general pattern of development of the several subdivisions of radiology. In the latter respect it is necessarily incomplete—some limit must be set on elaboration of detail. However, the sources cited are fairly extensive, and should provide an adequate bibliography for anyone interested in pursuing further some special interest.

Very little effort has been spent on evaluation of current status. Anything of that nature would become hopelessly out-of-date very fast, while predictions of the future in such a rapidly moving subject would be risky, to say the least.

I think all would agree that the better one is educated, the better he will function, whatever he does. Nowadays, with radiology reaching far out beyond its traditional bases in medicine and industry, everyone should be interested. Whether we realize it or not, all of us will be affected by radiology, in one way or another, sooner or later. The more we understand it and its history, the more effectively we can adjust to it.

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Modern Radiology
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PART I

THE FOUNDERS

Chapter 1

THE BACKGROUND PRIOR TO 1895

RADIOLOGY WAS ACTUALLY born in November, 1895 when Wilhelm Conrad Roentgen discovered his "new kind of rays." But just as in biography one must first consider a man's parents and background, so here it is necessary to see what the matrix was before and at the time of birth. As we shall find, the time was ripe, and the birth was a full term one. Considerable knowledge about the electro-magnetic spectrum was at hand, and x-rays had pretty certainly been produced by several physicists, without being recognized.

We are now going to explore the development of human thought and experiment which set the stage for Roentgen's momentous discovery in the last years of the Nineteenth Century. We will follow three principal converging paths: the atomic theory, the idea of transmutation of matter, and the physics of electricity, vacuums and photography.

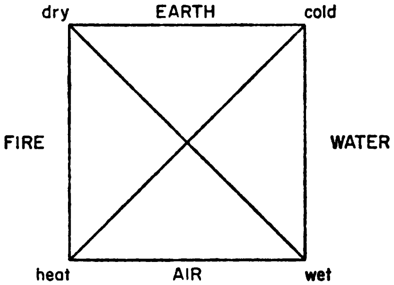
ATOMIC THEORY

It is a fascinating thing that the atomic theory, so basic to modern chemistry and physics, is such a very old idea. One feels that, even though the philosophical approach may have been entirely different, those keen minded ancient Greeks four or five centuries before Christ had something in common with the chemists and physicists of the Nineteenth and Twentieth Centuries.

One wonders how the notion ever arose in men's minds that matter might be composed of minute building blocks, in motion. Probably, it was observation of natural metamorphoses—such as wood to ashes through fire, and growth of vegetation from seed—that convinced even primitive minds of the reality of matter transmuting from one state to another. They could not, of course, distinguish chemical and physical changes from what we consider true transmutation. The obvious next step was to experiment, and so other metamorphoses were observed: solution of solids in liquids, evaporation and freezing of water, making deserts fertile through irrigation.

At first all this was just magic, or divine miracle. But gradually curious minds took what seems to me the really creative step of suggesting building blocks common to the transmuted materials. Here is where our earliest records begin. Men such as Thales of Miletus (650 B.C.), Anaximander (611 B.C.), Diogenes of Apollonia, Pythagoras, and Empedocles (all in the Fifth Century B.C.), began speculating that there were certain basic elements. These were: air, earth, fire, and water, and one or another was usually thought to be predominant. This general concept lasted a long time, being given added impetus and authority by Aristotle (Fourth Century B.C.), and never completely disappeared until the latter middle ages.

The relationship of these elements was graphically represented by this diagram:



The four basic elements are represented by the four sides of the square. Their intersections at the corners are marked by the qualities of heat, dryness, cold, and moisture. Interactions and transitions between these qualities follow the sides or diagonals to form any gradation or combination.

Biologic phenomena were related to each of the several elements, either alone or in combination. Disorders in nature, and specifically human illness, were thought due to their imbalance—such as irritability and anger resulting from too much fire.

The next major jump was to reduce polytheism to monotheism, multiple basic elements to one. Parmenides (Elea, Fifth Century

B.C.), appears to be the very earliest person to speculate in this direction, proposing an ill-defined "unity" as the "immutable basis of all matter." Leucippus took Parmenides' theory and added the idea that the units were small particles in motion.

A pupil of Leucippus, Democritus (Abdera, 460 B.C.) next emerges as a person about whom a little is known—some of his writings have survived. Most of these earlier men mentioned are known almost entirely through references to them by later authors. Democritus fashioned a consistent materialist theory which embodied the idea of conservation of mass. He took all matter to be composed of small "atoms" (the word comes from a Greek term meaning indivisible), and properties of matter to be due to the size, shape, arrangement and motion of these. Some of his ideas were not plausible in modern terms—such as the soul being a material thing, composed of the very finest and most active atoms—but it is rather for us to admire his significant achievement than to decry the imperfections.

In the Fourth and Third Centuries B.C., Epicurus, who has been immortalized through his philosophy and contributing his name to our language, accepted the atomic theory straight from Democritus without modification.

T. Lucretius Carus, the renowned Roman philosopher-poet, adopted the atomic theory during the First Century B.C., ascribing it to Epicurus, of whose school he was a follower. Lucretius' poem "de Rerum Naturae" has made his name a literary and philosophical landmark, since it has been preserved. He applied the theory in detail on a thoroughly materialist basis (and hostile to contemporary religious mysticism), but did not significantly add to Democritus' formulation.

From Lucretius' time on, till the turn of the Eighteenth to Nineteenth Century, no further development in atomic theory occurred. Speculation had gone as far as it could, and awaited the development of mathematics and technology to make it quantitatively convincing. It could not prove itself against competing ideas, though many active investigators gave it serious consideration: Frascatorius (1483-1533), Giordano Bruno (1548-1600), Gassendi (1592-1695), Galileo (1564-1642), and Boyle (1627-1691).

It remained for John Dalton (1766-1844), about 1800, to revive it as a significant explanation of the behavior of matter, and for Berzelius (1779-1848)—who gave credit to Dalton—to extend the concept in terms of modern chemistry, which was entering upon a rapidly expanding phase. Berzelius began to introduce a degree of quantitation to the theory, and developed the idea of atomic weights.

In 1815, Prout first suggested that hydrogen was the fundamental building block, and from then on till 1870 (when Mendeléev (1834-1907) introduced the periodic table), progress was extremely rapid in developing detail.

It is there that we shall leave this road, for we have traced atomic theory to our selected terminus for the time being. We shall pick it up later. Meanwhile we will turn back to the second path, and follow it.

TRANSMUTATION OF MATTER

When thought about atomic theory failed to progress after Lucretius' time no such lapse in men's interest in transmutation of matter occurred. This was a very practical—as well as philosophical—subject, and the direction of progress was in the line of experimentation. Much has been made of Aristotle's emphasis on art, beauty and harmony (drawing, of course, from Plato) as opposed to observation and experiment. His metaphysics, rather than physics, may well have acted to impede scientific thinking. Still, Aristotle's belief that the four basic elements were not fixed in their nature, but could blend or change, probably gave some push to the transmutation effort. Apparatus was crude, and materials impure. Thought was confused. But the impressive thing is that out of a maze of mystical nonsense there did begin to emerge, even in the first few centuries A.D. some rough ideas about chemical and physical properties of matter, and some pieces of fundamental equipment were developed.

At the time of Christ's birth, and for many centuries thereafter, all science could be, and usually was, summed up under the title of "natural philosophy." At its best this was sincere speculation and experimentation with matter; at its worst, selfish and avaricious desire to find an easy road to wealth and power. The term "alchemy" has come to be applied to the more or less chemical investigations prior to the modern period, a name derived from Arabic and supposed to allude to the black soil of Egypt. For Egypt, particularly Alexandria, was the chief locus of scientific thought in the early centuries of the Christian era.

We have discussed primitive intimations of transmutability of matter based (presumably) on observations of the metamorphoses of nature. It required little impetus for thoughtful men to pursue the idea on a theoretical plane, and for grasping ones to dream of achieving wealth by converting "base" metals into valuable ones. They tried hard. They did indeed have a store of empirical—but in some cases highly refined—experience in such arts as metallurgy, glass making, and medicinal botany to build upon. The comparison of spurious astrology (which, of course, still survives) to bona fide astronomy inevitably suggests itself, in thinking of the relationship of alchemy to modern chemistry and physics. After all, even astrology did amass a considerable body of valid observations and measurements.

The goals of alchemy may be listed as: 1) the transmutation of elements, 2) discovery of a universal cure for human ills, and 3) finding an elixir of youth. The key to these was supposed to be the "philosopher's stone." This "stone" was envisioned in entirely material terms, as a chemical of some sort, sometimes described as a red powder, and a universal solvent. The processes and results achieved were frequently not understood, and thinking was much clouded by applying mystical and anthropomorphic qualities and emotions to material elements.

There are surviving records from Thebes (Egypt) in the third century A.D. giving recipes for making gold from base metals. It is of interest here and later that a significant amount of effort in the alchemical endeavor was channeled into making counterfeit money—usually imitations of gold coins. Naturally this led to governmental interest. Many other substitute items were developed as side effects or pot boilers. Medicine often seized upon any new discovery, as it does even today, to apply empirically in the treatment of disease. Thus many alchemists practiced as physicians, at least part of the time.

The next authentic reference to alchemy appears when, in 290 A.D., the Roman Emperor Diocletian suppressed a rebellion in Egypt and immediately thereafter took pains to have collected and burned all books dealing with the manufacture of gold and silver from base metals. This policy was ascribed by a contemporary writer to fear of the Egyptians' regaining wealth and again rising in revolt. But Gibbon (5) inclined to the view that it was the Emperor's belief that the ideas of transmutation were quite foolish and should be suppressed as misleading. That was evidently Gibbon's feeling, and for this reason he applauded the book-burning as wise and prudent policy. Certainly the fact of persecution establishes the importance of an idea (though certainly not its validity), just as in many cases over the next few centuries official support and subsidy of alchemy indicated credence and hope of profit.

Alchemy survived both persecution and supervision. It grew fast about the Mediterranean, and made a considerable leap forward when the Arabs, full of vigor both intellectual and military, overran Egypt (640 A.D.) and the southern wing of the Roman world in the Seventh Century. The name "alchemy" was then born.

As education spread, so did this chief vessel of science. By the fourteenth century it had extended throughout the civilized world.

It may be of interest to skim over some of the famous personages who were active in alchemy, as well as a few significant historical landmarks. As in several other cases (gun powder, printing) scientific progress in China paralleled or preceded—independently—that in Europe. There is a Chinese manuscript of 140 A.D. giving alchemical recipes and procedures. Wei-po-yang (Second Century) was prominent in chemical work.

There is evidence that alchemy was practiced in India, in the eighth century, but here it probably spread eastward from Arabia.

Abu Musa Jabir (720-800), known to Europe as Geber, flourished about 776 A.D. and was apparently a leading figure in Arabian alchemy, but very little is reliably known about him. He and other Arabs (e.g., Avicenna and Rhazes, Seventh-Eighth Century) did a great deal of work and gave rise to numerous medical discoveries which gradually crept into Europe and substituted drug methods in many cases where the principal therapeutic tool of the physician had been phlebotomy.

Albertus Magnus (1206-1280), a Dominican priest and scholastic philosopher, was an outstanding and influential teacher at several European Universities. He taught Thomas Aquinas at Paris. He wrote extensively on philosophical and theological subjects, but was also interested in science. He gave encouragement to alchemy but made no discoveries. He wrote a book on the subject: *Libellus de Alchimia.*

Roger Bacon (1214-1294), was a Franciscan priest who taught at Oxford and was comparable to Albertus Magnus in fame and influence as a philosopher. He believed in the philosopher's stone and in transmutation. He conducted many careful experiments and set his face firmly in the direction of the modern experimental method.

In 1404, the English parliament passed a law forbidding manufacture of gold by means of alchemy.

Henry IV (regn. 1399-1413), and Henry VI (regn. 1422-1461) of England granted special permission to favored individuals to make gold.

Paracelsus (b. 1493), a Swiss, practiced as a physician, travelled widely, and accumulated most of the alchemical knowledge then available. He made several important chemical discoveries. He is supposed to have prepared laudanum, which we still use.

Oxford University was glad to accept, in 1677, the equipment and manuscripts of a well-to-do Englishman named Ashmole who experimented and wrote about the philosopher's stone. One condition of the gift was that a museum to house it be built. This was done, and the museum bears his name to this day.

Isaac Newton (1642-1727) who is, of course, remembered for his great work on classical mechanics, was interested in transmutation for a while.

Robert Boyle (1627-1691), who is famous for his work on gases, was also sufficiently interested in transmutation to work for the repeal of the law of 1404 forbidding alchemical production of gold.

Cagliostro (1743-1795), (real name: Guiseppe Balsamo), was certainly a colorful and adventurous man, perhaps brilliant, certainly unscrupulous. He travelled extensively about the Mediterranean and Europe collecting much knowledge, which he employed very practically by selling charms, philtres, and giving demonstrations. He was definitely a charlatan, and was finally imprisoned as a heretic.

There were, of course, many many other people, both famous and obscure, who contributed effort and imagination. Most, in fact, of the educated minds gave alchemy some attention. Quite a few prominent individuals (prelates, popes, princes, and physicians) were interested and active.

Finally, just to bring us up to date, it is worth reporting that in the first decade of the Twentieth Century one Stephen H. Emmens, right here in the United States, claimed to have discovered a modern philosopher's stone. This he called "argentaurum" and thought that with it transmutation could be accomplished. He actually sold to the United States government ten pounds of a metal he claimed to have transmuted. I have made some effort to find out about this transaction through our National Archives in Washington. The records are very scanty, and about all that has turned up is that the government finally regarded the whole thing as a deliberate fraud.

What did alchemy achieve? Not any philosopher's stone, nor transmutation as we conceive it today. But it did keep alive the idea, and along the way piled up an astonishing amount of miscellaneous chemical and medicinal lore. That the idea of transmutation fell into disrepute is an irony of fate. Vindication finally arrived through the hands of the very science which condemned the alchemist's dream.

Looking back over the centuries it becomes evident that alchemy began to split fairly early, probably in the time of Roger Bacon and Albertus Magnus. The more progressive wing concerned itself with what came later to be modern science, gradually building solid fact on solid fact. More examples of this type of research will presently emerge as we get into more modern physics. Indeed, one very pertinent example concerns fluorescence (6) which, as we shall see, played a dramatic part in Roentgen's very first recognition of x-rays. Fluorescence is one division of the larger phenomenon of luminescence. Luminescence is divided into fluorescence (in which light is emitted only during excitation by some energy source) and phosphorescence (in which light continues to be emitted after cessation of the energy input). The name fluorescence derives from fluorspar, a calcium fluoride, and one of the chief commercial sources of fluorine.

In 1570, a Spanish physician named Niccolo Monardes observed light emitted by a tincture of a certain wood, lignum nephriticum. Others during the succeeding century studied this phenomenon further.

In 1602, Vincenzo Cascariolo, a cobbler of Bologna, who was a part-time, do-it-yourself alchemist, picked up a rock on Mount Pesara which sparkled in the sun. He took it home and cooked it in his furnace, hoping to derive some valuable mineral from it. During his manipulations he found that it shone in the dark after being exposed to light. This substance achieved some celebrity under the name of "Bologna Stone." Because it was porous it was thought to store up sunlight like a sponge, and then emit the light later in the dark.

Many more experiments on fluorescence and phosphorescence were made over the next three centuries, some of them by famous personages like Sir Isaac Newton, and a certain Henri Becquerel, whom we shall hear about in some detail later. By the latter nineteenth century fluorescence was in common use by physicists as a tool to study light and other energy sources.

The less progressive wing of alchemy clung to the magical aspects of it and paid the price of withering away. By the seventeenth century the achievements of bona fide chemistry and physics were beginning to occupy the keener minds, and the fanciful or selfish aims of alchemy became less and less fashionable. From then on the rise of modern science took and maintained the initiative more and more. Although, as mentioned, alchemy survived to the twentieth century, it was effectively superseded during the eighteenth.

Still, it is well for us to reflect for a moment that those many men, working with what tools and experience they had, were among the precursors of our modern physics and radiology.

MODERN PHYSICS

The third path I have chosen to follow is really a broad highway and deals with that same rise of modern science just alluded to. This science covered a very wide scope, of course, but we are concerned here with just three special fields which were necessary to the discovery of x-rays: vacuums, electricity, and photography. These we shall trace individually.

Vacuums

The idea of a completely empty space has always bothered men. "Nature abhors a vacuum" summed up the attitude of the ancient Greeks, and this term was crystallized by Spinoza (1632-1677) in his *Ethics* (Part I, Prop. XV). It seems probable that it was men rather than nature which abhorred the vacuum.

Lucretius, however, envisioned material atoms in motion in a "void," and this is very close to modern concept, so the degree of repugnance to emptiness may well vary with one's familiarity with, and acceptance of, atomic theory.

Of course, in gross Newtonian physical terms, nature does abhor a vacuum, and how this abhorrence was overcome is what we are to consider.

Probably the first vacuum produced and recognized as such was when Evangelista Torricelli (1608-1647), a pupil of Galileo during the latter's last years, invented the barometer in 1643 by inverting his glass tube full of mercury, and stood it upright in a vessel of the same. Clearly, the space above the mercury column must be empty.

Otto von Guericke (1602-1686) was a colorful figure who next appears upon the scene. He was obviously an outstanding person intellectually, and although he dabbled in many scientific researches it is interesting that these pointed forward toward modern physics, and had little to do with alchemy. He studied law and science at Leipzig, Jena, and Leyden. He was forced to flee his native city of Magdeburg in 1631 by Tilly's brutal sack of the town during the Thirty Years War, and entered Swedish military service for several years. He returned to Magdeburg and by applying his talents to politics, he became burgomaster in 1646. Now he had some leisure and financial comfort to pursue scientific experiments, and we can easily imagine that he had a grand time. He independently duplicated much of Torricelli's work, and constructed a working water barometer. Having become interested in the idea of a vacuum, he developed (in about 1646) the achievement in which we are now interested: his air pump, by which he could exhaust a good part of the air from a tight vessel. His showmanship in the famous exhibit of the "Magdeburg Hemispheres" in 1654 reached a high point of laboratory demonstration for all subsequent teachers to envy and strive for. In this experiment before the Emperor Ferdinand III and many notables at Regensburg, two teams of eight horses each were unable to separate two closely fitting metal hemispheres held together solely by the vacuum within them. When the valves were opened and the vacuum disappeared, the hemispheres fell apart spontaneously.

The principle was established. From then on many refinements of technique were added (notably by Robert Boyle in 1659 and Herman Sprengel in 1865) permitting higher and higher degrees of evacuation. Vacuums thus made available were seized upon by other scientists for experimentation, leading directly to those explorations of the behaviour of electric currents *in vacuo* presently to be mentioned.

Electricity

Here we must again cast back to antiquity, as static electricity was recognized by the attractive power derived from friction of such materials as amber and fur by the ancient Greeks: Thales (600 B.C.) and Theophrastus (321 B.C.). The phenomenon remained merely a curiosity, however, until picked up again in the Sixteenth Century.

Magnetism was recognized too, in natural magnets, or "lodestones," in distant antiquity. The mineral magnetite ($Fe_3 0_4$) is so named from the city of Magnesia, in Asia Minor, near which many lodestones were found. The compass is a special case of a natural magnet aligning itself to the earth's magnetic poles. The date and author of the discovery of the compass is uncertain, and it is thought to have been discovered independently in China and Europe. Compasses were in use in both areas by the latter half of the Thirteenth Century.

William Gilbert, of Colchester, England (1540-1603) is really the first man to delve into electricity and magnetism seriously. He turned his back on alchemy and proceeded to develop many experiments, building painstaking observations patiently upon already established data. He was well educated (Cambridge) and practiced as a physician. For our purposes it is important chiefly that he observed and studied static electricity in detail, coined the term "electric," and invented a primitive electroscope. It is also noteworthy that he studied magnetism intensively, and recognized for the first time that the earth itself is a great magnet.

Our friend Otto von Guericke, who lived just after Gilbert (1602-1686) must be mentioned again, as he was much impressed with Gilbert's work, carried some of it further, and in particular developed a static machine which produced sparks and phosphorescent light. It has been suggested that thus he was the first man to witness electric light phenomena.

From the Seventeenth Century on, progress snowballed. More and more men entered upon careers in science; there was much work done duplicating and verifying experiments; and of the many investigators only the most exceptional can be mentioned. In order to cover the ground it is necessary to adopt at this point a more synoptic than narrative form.

Robert Boyle (1627-1691) conducted further experiments with electricity.

Isaac Newton (1643-1727) built improved static generators about 1675.

Francis Hauksbee (?-1713) conducted further studies on static electricity. He built his own generator; indeed most investigators until much later found it necessary to make their own equipment. He observed mercurial phosphorescence and electrical discharges of light *in vacuo,* particularly sparks, and brush discharges.

Charles DuFay (1699-1739) distinguished two different kinds of electricity. One he described as "vitreous," when glass was rubbed with silk; the other "resinous," when silk or paper was stroked with wool.

Abbé Jean Antoine Nollet (1700-1770) constructed an improved electroscope. Because of its ovoid shape this was called his "electric egg," and being a vessel for discharge of electricity under conditions of reduced pressure was in a sense a forerunner of the x-ray tube.

Benjamin Franklin (1706-1790) conducted many electrical experiments and is particularly remembered for his demonstration of the identity of electricity and atmospheric lightning. He developed a lightning rod. He was the first to use the term "plus" and "minus" to distinguish the two kinds of static electricity. One picturesque anecdote about him illustrates his sense of humor. He and a group of fellow scientists dined on turkey killed by electricity and cooked by electricity. Toasts to famous scientists in Europe were drunk in "electrified" glasses, accompanied by loud reports from electrostatic generators!

E. J. von Kleist (1715-1759) in Germany, and two professors at Leyden (Cuneaus and van Mussenbroeck, 1692-1761), independently and at the same time developed jars which were variously known as Kleist jars and Leyden jars, capable of collecting and storing static electricity. These jars were widely used in research and for showy exhibits. Nollet particularly liked to stage elegant demonstrations, such as making a row of soldiers holding hands jump simultaneously upon administering a sizable shock to the man at the end of the row. Attempts were also made to utilize electric shocks in medicine, but no solid results followed—until two hundred years later, when electro-shock therapy became important in psychiatry.

William Watson (1715-1787) transmitted electricity from a Leyden jar through wires and through elongated vacuum tubes. Here we see the beginnings of current electricity as contrasted with static.

William Morgan (?-1785) observed changing colors during electrical discharges in partially evacuated tubes. This observation was partially accidental since at first his Torricelli vacuum was so high that no discharge occurred. When a tube cracked, however, and some air leaked in, the colors appeared. He suggested estimating the degree of vacuum according to the colors displayed. It is possible that he may inadvertently have produced x-rays.

Luigi Galvani (1737-98) observed an isolated frog muscle preparation twitch under an electric stimulus. He incorrectly attributed the source of the stimulus to the tissue itself.

Alessandro Volta (1745-1827) was an extremely talented Italian physicist. He was a professor of physics at age twenty-nine, travelled widely in Europe, and made many contacts with contemporaries. He discovered in 1798 that electricity is created whenever two different conducting materials are in contact; this was the source of the electricity in Galvani's observation. In 1800, he developed the "Volta pile," the first battery and prototype of all future ones. It was a practical application of the discovery of production of electricity through contact of different metals. This battery is notable as being the first new source of electricity, differing from the static machines which had been the only source before.

Hans Oerstedt (1777-1851) discovered in 1820 the relationship between electricity and magnetism.

André Ampère (1775-1836) distinguished current from potential energy.

Georg Ohm (1787-1854) propounded the famous law known by his name stating the relationship between potential, current, and resistance. He also experimented with thermocouples.

Michael Faraday (1791-1867) was the son of an English blacksmith and was largely self-educated. While working for a bookbinder, he took advantage of the opportunity for wide reading. As a boy he was introduced to Sir Humphry Davy who befriended him and gave him a job. In 1821, he conducted his famous experiment of demonstrating rotation of a magnetic needle about a wire carrying electric current. In 1831 he induced an electric current by moving a magnet in and out of a coil, and coined the term "electromagnetic induction." These discoveries led directly to generators, motors and transformers, equipment that rendered possible higher voltages than ever before. Faraday also began the study of high voltage discharges in evacuated tubes, but this field was carried considerably further by later investigators.

Joseph Henry (1797-1878) had a career which closely paralleled that of Faraday. From humble beginnings near Albany, New York, he earned his own education and early displayed great talent in scientific research. In 1832, he was called to Princeton University to teach, and there conducted many experiments on electricity. He actually succeeded in inducing an electric current independently. Although this discovery and others duplicated much of Faraday's work, the latter had more publicity and has received most of the credit. When I was a student at Princeton I had the opportunity of seeing in the Physics Department museum some of Henry's apparatus. One homely detail sticks in my mind: he used some of Mrs. Henry's silk underclothing for purposes of insulation. This will touch a responsive chord in those who have to worry about department budgets. He also strung wires and sent signals between the physics laboratory and his house, a distance of about one-quarter of a mile. In 1846 he was made director of the Smithsonian Institution, continuing there many further experiments.

Yet another man, given even less publicity, also during the same period duplicated the feat of discovering induction. He was an Italian, named Zantedeschi.

G. Belli, in 1831, made significant improvements on existing static machines, and further advances were made by several men during the Nineteenth Century, particularly Wimshurst in 1860. Even though the Wimshurst apparatus was further improved by Holtz in 1880, the name Wimshurst survived and was familiar to early roentgenologists as a standard source of static electricity.

Following Faraday many improvements on induction coils were also made by numerous workers. The most notable of these was H. D. Ruhmkorff of Paris. His name became attached to the best induction coils and was a familiar term for them up to the early years of the Twentieth Century.

James Clerk Maxwell (1831-1879) graduated from Cambridge when Faraday was sixty-three years old. He became deeply interested in electricity, and his most important work lay in reducing electrical theory to mathematical terms. This finally led to the electromagnetic theory of light. Some of his predictions were verified only after his death.

Heinrich Hertz (1857-1894) was an extremely talented teacher and did much further electrical research.

Julius Pluecker (1801-1868), using tubes made by H. Geissler, studied many effects of electrical discharges in gases at low pressures. He observed cathode rays.

Wilhelm Hittorf (1824-1914) observed and studied cathode rays further.

William Crookes (1832-1919) studied cathode rays and demonstrated that actual matter was emitted from the cathode. He also showed that this had enough energy to rotate a wheel placed within the tube. He repeated much of Hittorf's work and because of greater publicity received most of the credit.

H. von Helmholtz (1821-1894) was an influential physicist who did important work on electromagnetic theory, and predicted theoretically some of the properties of x-rays. Among other accomplishments, he invented the ophthalmoscope.

Philipp Lenard (1862-1947) further investigated cathode rays, continuing the work of his teacher, Hertz. He demonstrated cathode rays penetrating thin metal sheets and progressing a few centimeters in air. He found that cathode rays affect photographic emulsions, and used an aluminum "ladder" to measure and analyze their penetration. He also studied the deflection of these rays by magnetic fields.

This is the point at which physical knowledge stood when Wilhelm C. Roentgen was making his experiments with cathode rays in November, 1895.

Near the beginning of this chapter we noted that x-rays had been produced by other investigators prior to Roentgen. Of course, with voltages available these would have been rather feeble. The following men's work and records suggest more or less definitely that they were hovering close to the brink of discovery. Francis Hauksbee (early Eighteenth Century) (11), and William Morgan (latter Eighteenth Century) (8) certainly lacked the means to recognize and develop x-rays. But such eminent physicists as

Pluecker, Hittorf, Crookes, Lenard, and Stine in the decade preceding 1895 had most of the tools that Roentgen did. Indeed, William Goodspeed in 1890 definitely produced a radiograph, and recognized it in retrospect, after Roentgen's discovery.

Photography

It remains now to detail the last of the subjects we are to consider, photography. Although this may seem a less significant field than theoretical physics, it is worth repeating that theory without technique cannot get very far. The photographic recording of otherwise invisible rays was an extremely important instrument in the hands of investigators immediately preceding Roentgen, and was also very valuable to him. A Nineteenth Century astronomer is quoted as follows: "The photographic plate is the retina of science," and indeed the method is invaluable to most branches of science.

The basis of photography consists in the sensitivity to light, cathode rays, or roentgen rays, of certain silver salts. We need not go into the details of this chemical reaction.

As early as 1727, J. H. Scholtz made the first photographic copy of some written material. In 1802, Wedgwood produced copies of pictures. In 1839, L. J. Daguèrre developed an improved silver plate sensitized with iodine. Photography became more practical, received considerable interest, and the technique spread. "Daguerrotypes" became popular.

Improvements were made by J. W. Draper in 1840, by H. F. Talbot in 1841, and by Archer in 1851. In 1871 R. L. Maddox produced a gelatin silver bromide emulsion which has remained the basis of modern photographic work. In 1884, George Eastman patented roll paper film, and in 1887, H. Goodman patented a roll of celluloid film. With this equipment, and improvements in cameras developed from well known principles of optics, modern photography went on to many refinements upon the basic principles.

I have tried to trace rapidly the development of the major fields of knowledge concerned in the background of x-rays and radio-activity. In concluding this chapter I want to emphasize that, although we have mentioned many names of famous men, we should also remember the army of others who contributed less noticeably, but nevertheless significantly to the advance of knowledge. Some of these may be known as the teachers, pupils, assistants and contemporaries of the famous. Many others are not known. To this end I pass on two quotations:

> *"Who knows whether the best of men be known, or whether there be not more remarkable persons forgot than any that stand remembered in the known account of time?"*
>
> SIR THOMAS BROWNE

> *"Not a day passes over the earth but men and women of no note do great deeds, speak great words, and suffer noble sorrows. Of these obscure heroes . . . the greater part will never be known till that hour when many that are great shall be small, and the small great."*
>
> CHARLES READE

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Chapter 2

WILHELM CONRAD ROENTGEN

"In the history of Science, nothing is more true than that the discoverer, even the greatest discoverer, is but the descendent of his scientific forefathers; he is always essentially the product of the age in which he is born."

(SYLVANUS P. THOMPSON, THE ROENTGEN SOCIETY, LONDON, Nov. 5, 1897)

ROENTGEN WAS WELL aware of contemporary investigative work in physics. Much of his time was spent reading the literature and corresponding with colleagues. He gave generous credit to all of his predecessors, and particularly to Lenard for suggesting promising leads.

In digging into Roentgen's personal background, I was interested to find that he was color-blind. One may speculate lightly that this might have something to do with radiology being a field of black and white shadows. We have not yet, at least, followed the automobile industry and developed "two-tone" x-rays. Actually this handicap did not interfere with Roentgen's ability at all, even though he had the additional affliction of limited vision in one eye, the result of a childhood illness.

Wilhelm Conrad Roentgen was born on March 27, 1845 in Lennep, a fair-sized town near the Rhine, at the edge of the Ruhr valley. He was the only son of a textile merchant whose ancestors had lived in or near Lennep for many generations. Most of them had been skilled artisans or small business men. His mother (a cousin to his father) stemmed also from Lennep, but her family had lived in Amsterdam for a few generations, engaged in shipping and small business. Neither family boasted any scientists or preeminent scholars.

When he was three years old Roentgen was taken with his family to Apeldoorn in Holland, and they became Dutch citizens. The question of whether this move had any relation to the civil disturbances of 1848 is not settled.

Roentgen's childhood was uneventful, in well-to-do circumstances and cultured surroundings. He attended several different schools, the most important of which was the Utrecht Technical School, which he entered in 1862 at the age of seventeen. It may be of some interest that he roomed in the house of a well-known chemist named Gunning who taught at the school, and Roentgen was rather impressed and stimulated by him. As a student his performance was average and he enjoyed the usual extracurricular outdoor activities. He left the school in 1864, being expelled when caught admiring a caricature of one of the teachers drawn by another pupil. He refused to tell the name of the culprit. This anecdote struck me particularly since my own experience in a Bavarian public school exposed me to a somewhat different tradition. At my school, in 1931, it was considered proper and honorable for pupils to inform the teacher of any mischief done. It seemed to be a result of rigorous training in the touted German passion for law and order, with devotion to authority transcending any personal loyalty.

Expulsion from the school cut Roentgen off from the orderly and rigid educational ladder. However, he obtained special permission to prepare privately for the "absolutorium" (a certificate comparable to our college entrance examinations), and did so. He failed the examination, and it has been suggested that this may in part have been due to an unsympathetic examiner, a man who had been involved in the unpleasant episode at the Utrecht school. Thus the path to advanced education was again blocked.

His family was anxious to promote his education, even though privately hoping that he would follow his father's career as a merchant. In order to lose as little time as possible he registered at the University of Utrecht in January, 1865, as a special student (without credit), and for two semesters attended lectures on philosophy and various scientific subjects.

During 1865 he was told by a friend about the Zürich Polytechnic School, which had its own rather stiff entrance requirements but did not require other formal credentials. He applied at once, expressing a desire to devote himself to applied mathematics. On review of his experience examinations were waived and he was admitted.

For three years he studied mathematics, mechanical drawing, engineering, metallurgy, thermodynamics and other related fields. The school was a good one and many of the faculty were the best teachers of the period. In August, 1868, he received his diploma as a mechanical engineer, with excellent grades. His interest was already shifting, however, to basic—as opposed to applied—science. Nevertheless, the practical experience in engineering fields was of great value to him throughout his life. After graduation Roentgen remained at Zürich to take additional courses in mathematics. In particular he studied under August Kundt, applying himself to the field of Kundt's chief interest, the theory of light. In Kundt's laboratory he did experiments on gases which were the basis for his thesis in 1869 for the degree of doctor of philosophy (granted in June of that year). During this last period the conviction began to crystallize that physics was his métier.

The life in Zürich was not all grinding at books. He enjoyed mountain climbing, and the Swiss mountains exerted a life-long fascination for him. Almost every year he managed to spend some time in these beloved surroundings. Boating parties on the lakes and jolly gatherings with other students were common. At a nearby inn, "Zum Gruenen Glas" he met and formed a friendship with the host, one Johann Gottfried Ludwig, a political refugee from Jena. Ludwig was an educated man, and a fairly intellectual atmosphere pervaded the inn, which was commonly frequented by students. Here Roentgen met Ludwig's daughter Bertha and the pair rapidly became fast friends. They were engaged in 1869.

After receiving his degree, Kundt asked Roentgen to remain as his assistant. They worked very well together for several years with the sole exception of one bitter fight when Roentgen dared to touch some private, exceptionally valuable laboratory equipment of Kundt's.

In 1870, Kundt was called to the chair of physics at Würzburg and took Roentgen along as his assistant.

In January, 1872, Roentgen married Bertha Ludwig at Apeldoorn. They embarked upon the rather bleak life of a poorly paid physics assistant, which must have been difficult for two people brought up in comfortable surroundings.

At Würzburg work was subject to the drawbacks of cramped quarters and inadequate equipment. In particular, Roentgen was disappointed that his Swiss degrees were not accepted and he could not be appointed to any position of recognized academic standing. Once again he was caught in the meshes of a rigid educational system, just as he had been in Holland.

Later in 1872, Kundt moved on to the University of Strassburg, newly rejuvenated after the Franco-Prussian war. Here conditions were much better and in a more liberal atmosphere Roentgen received academic recognition as "Privat-Dozent" in March, 1874. Now the barriers to academic progress were down, and he became more ambitious. He worked very hard as Kundt's assistant and was active in teaching.

In April, 1875, the lure of a position as head of a department caused him to accept a call to the Agricultural Academy at Hohenheim in Württemberg, even though this involved leaving Kundt and many friends in Strassburg. His parents had moved to Strassburg also during his residence there.

The privilege of being chief, however, proved somewhat hollow, since facilities at Hohenheim were exceedingly poor, and one and one half years later he was glad indeed for the opportunity to return to Strassburg as associate professor of theoretical physics.

Here he continued teaching and research in collaboration with Kundt. Two of fifteen papers published during his Strassburg years were on the subject of discharges of electricity through conductors and insulators. He continued to gain experience in teaching and technique. He developed clean-cut methods, skill at demonstration, and his conclusions were always well controlled. Most of his quantitative observations were never improved upon. Inevitably his reputation grew.

In 1879, the chair of physics at the University of Giessen was offered him, and at the age of thirty-four he accepted. Here he found a happy situation, stimulating friends, and continued his studies on gas, heat, and crystals. The facilities were not good compared to Strassburg, but he obtained a new physics building in 1880. In nine years at Giessen he continued active teaching and research, and published some eighteen papers on a variety of subjects. His interest moved rapidly from one idea to another and he was sometimes too impatient to write up a specific project. He developed acquaintance with outstanding contemporaries such as Hertz and Lenard. He was an avid reader of the scientific literature and kept himself extremely well informed.

While at Giessen he became financially independent for the first time and one symptom of luxury he permitted himself was the renting of hunting grounds and construction of a small cottage there. A source of considerable grief to him was the death of his parents during this period. He was always extremely attached to his family and because he and Bertha had not been able to have children they adopted a niece of Bertha's. In 1886, an offer from the University of Jena was received and in 1888, another from the University of Utrecht. Both of these were declined. One may wonder what his feelings were upon being offered a professorship from the Dutch University which he had been unable to enter as a young student lacking credentials.

In October, 1888, at the age of forty-three, he received an offer from the University of Würzburg which was so attractive that he felt compelled to accept it. This included a new physics institute with extremely good facilities. The return to Würzburg was in the nature of a triumph compared to his earlier humble sojourn there. He was fêted and accepted with honor by all of the prominent people.

During the next six years he published some seventeen papers and continued to develop stature as a teacher and experimental worker. One may say that Roentgen was now in his prime.

In 1894, he was elected Rector of the University, a post corresponding to chancellor or president but without most of the administrative duties commonly associated with that post in the United States. Thus it was possible for him to continue his work as head of the physics department, and his own personal research.

During the year 1894, he was greatly saddened by the deaths of Hertz, Kundt, and Helmholtz, with all of whom he had had close personal ties.

In early 1895, he received an invitation from the University of Freiburg which was declined.

During the autumn of 1895, he became deeply interested in the cathode ray experiments of Hittorf, Crookes, Hertz, and Lenard, and concentrated his attention on them. He repeated many of their experiments while expanding his interest. He appears not to have taken up the study of cathode rays very seriously before this point. In early November, at this time in his fifty-first year, Roentgen set about making special laboratory arrangements for space for his cathode ray experiments. He had several Hittorf-Crookes tubes, as well as a very good Lenard tube. His source of electricity was a large Ruhmkorff induction coil with a Deprez interruptor; this produced sparks four to six inches long. He had his own vacuum pump for the purpose of evacuating tubes. Most of this original equipment is on display at the Deutsches Museum in Munich. As a boy I was fascinated by this wonderful museum and spent many happy hours there in the winter of 1930-31. It gives me a thrill now to remember that I saw this same Ruhmkorff coil, etc., though then I took it in my stride, not understanding it and knowing nothing of Roentgen at the time.

He repeated Lenard's demonstration of fluorescence of barium platinocyanide outside of the vacuum tube due to the rays passing through a thin aluminum window. It occurred to him to try the same thing with a Hittorf tube containing no such window. On November 8, 1895, in a darkened room, late in the day, when the lab was empty, he passed an electrical discharge through his Hittorf tube which was shielded completely with black cardboard so that no light from fluorescence within the tube could be seen. He tested the cardboard shielding for light tightness. As he was about to test for penetration of cathode rays through the wall of the tube he suddenly noticed in the darkened room a patch of light on a bench a few feet away. He soon discovered that it was his barium platinocyanide screen lying there and by turning on and off the current he verified that the fluorescence of the screen was related to discharge within the tube. This was a startling phenomenon since the fluorescence observed could not be due to cathode rays—they simply did not travel that far. Here he took the creative step. Instead of shrugging off the phenomenon as some freakish experimental error (as others had already done before), he had sufficient confidence in his own accuracy of observation to realize that this was something that required explanation. Reason told him that it must be something emanating from the vacuum tube, and this something was completely new. The situation was one which characteristically fascinated him. He remained at work, repeating the experiment with variations for hours. His diener came and went from the laboratory without being noticed. Frau Roentgen sent a servant several times to call him to dinner. When he finally came he ate little and remained in abstracted silence. He hurried through the meal and returned to the laboratory. When his wife asked him what was the matter, she received no reply and thought that he was in a fit of bad temper. More and more interested, he continued feverishly at work, and actually lived in his laboratory for the next few weeks.

One of his first ideas, since he observed the penetration of the beam through air, was to test its penetration through various materials. He tried a sheet of paper, a pack of cards, a book, and various metal objects. Finally, while holding a disk of lead he was really startled to see the bones of his own hand. One may imagine that he was somewhat shaken.

He continued working through December, and having discovered the effect of the rays on a photographic plate, he recorded many observations of the differential development according to the densities of various objects. He photographed an image of his wife's hand with a fifteen minute exposure. The experience of seeing her own bones was particularly horrifying to Frau Roentgen who, like many others subsequently, saw in the skeleton a premonition of death. By Christmas he had carried investigation far enough to feel that the time was ripe for publication. He pulled his observations together and submitted the first paper on "A New Kind of Rays" to the Würzburg Physical-Medical Society on December 28, 1895. He made the unusual request to have the paper printed in the "Transactions" of the society without having been formally delivered as a lecture.

Since this first paper was such a landmark, it is worth examining in some detail. It contains the salient information on the means of producing x-rays, as well as their most important physical properties. He described the passage of the beam through various media and recognized density as the most significant factor in absorption of the beam. He demonstrated differential transmission of rays by means of a ladder of tin strips. He also showed the photographic effect of the rays and recognized the hazard to unexposed film. He considered the possibility that the rays were cathode rays or ultra-violet light and ruled these out because there was no magnetic deflection of the beam, the distance was completely beyond the range of cathode rays, no refraction could be demonstrated, no reflection of the beam occurred, and no polarization was possible. However, the features common to x-rays and visible light (formation of shadows, fluorescence, and photographic chemical effects), were sufficient to convince him that he was dealing with some *rays,* and he suggested the possibility that they were longitudinal vibrations. This brings up a subject of contemporary discussion since many scientists in the latter nineteenth century were arguing about whether or not there were such things as longitudinal vibrations in the "ether." Roentgen observed attenuation of the beam according to the inverse square law and believed that x-rays were produced in the wall of the tube by the action of the cathode rays.

The paper was accepted, and was indeed printed at the end of the 1895 volume of the "Transactions." This rapidity of action stands out in sharp contrast with our usual present day experience of publishing delays! Reprints were available to Roentgen to mail out on January 1st, 1896 to his friends and many important physicists. He was under quite some anxiety about what reaction might be stirred up. Such a novel discovery might have led to some incredulity and perhaps ridicule.

Within a few days, however, he was receiving excited congratulations from his fellow scientists. In a few more days newspapers were trumpeting the news all over the world. Warburg, in Berlin, hastened to enter Roentgen's reprint and some specimen photographs in an exhibit at the fiftieth anniversary of the Berlin Physical Society. Here considerable publicity was obtained. A reprint sent to Roentgen's friend Exner in Vienna circulated among other friends, and the father of one of them was the editor of the Vienna Presse, which printed an enthusiastic article on January 5th, 1896. This was picked up by the Frankfurter Zeitung, which on January 7th, announced a "sensational discovery" in a style of popular science which has become familiar to us today. The newspaper writer even suggested medical and surgical applications—and this within one week of publication, before any physicians had had a chance to be heard from!

Similar more or less sensational articles appeared in newspapers all over the world during the next few weeks. It was only on January 9th, that the local Würzburg Generalanzeiger picked up the story. On January 13th, it was published in Paris (Le Matin), and on January 16th, in the New York Times. By mid-February seven articles on the subject of x-ray had appeared in European and American medical journals (including *The Journal of the American Medical Association,* which published an editorial on February 15, 1896, in which therapeutic possibilities of x-rays were already suggested). Some four more articles had appeared in nonmedical scientific journals. It is amusing by way of contrast, that in March, despite the international tumult on the matter, the Vienna police refused permission for a public lecture on the subject of x-ray because "no details about it have been learned by this office." And indeed, the Vienna police had no monopoly on blind spots. During 1896, and the next few years, even physicists continued to argue over, doubt, and rediscover fundamental properties of x-rays clearly enunciated by Roentgen in his first and second papers on the subject.

It is certain that Roentgen's clean-cut observations and his known reputation for reliable experimental work convinced the scientists who received his first reprints. Probably, however, the radiographs of hands which were rapidly circulated and duplicated elsewhere had a lot to do with catching the imagination of the nonacademic world. In particular, the interest of the medical community was naturally stirred. It is conceivable that without such a dramatic demonstration, x-rays might have remained quietly in the academic domain of physics for at least a short while.

Roentgen's personal life was thrown into complete disorder. Visitors and letters came in large numbers. He was harried by newspaper men, had no privacy, photographs and other objects were stolen from the laboratory, and even post cards signed with his name were stolen from the mails. Frau Roentgen's distress at the disturbance may readily be imagined.

The discovery was not long in reaching official government notice. He was summoned to demonstrate his discovery to Kaiser Wilhelm II on January 13th, and was decorated.

On January 23rd, he yielded to the insistent demands of his colleagues and broke his rule against personal publicity. He gave a public lecture at Würzburg University, indeed in his own building. He described to his distinguished audience how he had made his discovery and followed it up. It is interesting to consider his words when describing his observation of fluorescence due to x-rays: "but I still believed I was the victim of deception. Finally I used photography, and the experiment was successfully culminated." This reminds us of many situations nowadays where we often feel the need to confirm our fluoroscopic observations by means of the permanent record of a roentgenogram. At this lecture he publicly made an x-ray of the hand of von Kölliker, a prominent anatomist at the university. The effect upon the audience of taking the radiograph and presenting it developed some minutes later was of course sensational. Copies of this photograph were later peddled to the public at 50 Pfennig. Von Kölliker was himself greatly impressed and gave a brief discussion of possibilities of using x-rays in anatomy and medicine. He concluded by proposing for the first time the name "Roentgen rays" instead of x-rays. This proposal was repugnant to Roentgen and he opposed it throughout his life—obviously without success.

On March 3rd, his University awarded him an honorary Doctorate of Medicine and the Würzburg students gave him what is a very signal honor at German universities, a torchlight parade.

We might pause a moment to consider that the degree "Medicinae Doctor" is one extremely rarely granted *honoris causa.* Rare as it is, I think we can agree that seldom before or since has the honor been as richly deserved.

Along with the flood of applause there came, inevitably, a few sour notes. Some people were alarmed at possible dangers, though the real ones that we recognize today were not at that time known. Some efforts to minimize Roentgen's discovery were made, and it distressed Roentgen extremely that Lenard (who had been very friendly before) turned against him. Lenard, who was definitely an outstanding scientist, and who received his own meed of credit with a Nobel prize in physics in 1906, continued to make veiled hints of having discovered x-rays before Roentgen, even long after Roentgen's death. But he never made any public, official claim. For a long time he clung to the notion that the phenomena observed were all due to "his" cathode rays.

Efforts were made to suggest that the discovery was made merely by chance. Experiences of others were resurrected, notably some strange photographic effects in Crookes' laboratory in 1890 and an accidental photographic exposure noted by Goodspeed in Philadelphia, also in 1890. Another man, a professor of physics named W. M. Stine had hovered on the edge of discovering x-rays in 1892 while working at Ohio University. His experimental work had been interrupted by personal problems, but he was tantalizingly close. The key fact, however, is that these men at the time shrugged the novelty off while Roentgen recognized something new and nailed it down.

There seems to be a fairly widely held attitude among many scientists, particularly those in academic work, that the truly classic and beautiful experiment is the one in which a hypothesis is erected and an elaborate plan developed, which may involve months of preparation of much complicated equipment with many adjustments for all possible errors. The observation then may take a few minutes and the experiment is completed. People with this feeling sometimes deprecate the discoverer who turns up at random, seemingly by chance, or serendipity, a new fact which he was not deliberately looking for. It seems clear to me that these are entirely different modes of approach, and neither one has any right to criticize the other. The discoverers of fundamentally new phenomena or laws necessarily operate differently from those who take these findings and amplify them. Specifically, the latter are usually pursuing a very definite project, and tend rigidly to exclude from their attention any irrelevant phenomena—thus for the sake of concentration putting on blinders. Furthermore, the chance or opportunity for discovery comes to many of us but the man who recognizes and seizes it is surely worthy of credit for his particular gift of recognition. I have a strong feeling that people who are lucky make a large part of their own luck. A professor from Harvard, Munsterberg, who was in Germany at the time of the discovery summed up the situation succinctly. He said: "Suppose chance helped. There were many galvanic effects in the world before Galvani saw by chance the contraction of a frog's leg . . . The world is always full of such chances, and only the Galvanis and the Roentgens are few." Another case in point is the more recent "chance" observation by Pavel Cherenkov of the radiation that bears his name, and for which he was honored with a Nobel physics prize in 1958.

From an altogether different standpoint another latter nineteenth century comment on methods of research has seemed to me pertinent. W. Ostwald wrote: "The Romanticist produces quickly and much . . . . The Classicist generally shows aversion to extempore instruction. While it is the Romanticist's first concern to finish the current problem so as to make way for a new one, the Classicist is chiefly concerned with dealing with the current problem thoroughly and exhaustively . . . . the works of the Classicist endure much longer." Roentgen was emphatically a classicist. Control of results by ever more rigorous tests was a passion with him.

By the ninth of March, 1896, Roentgen was prepared to submit a second paper on further observations of the properties of x-rays. This was also delivered to the Physical-Medical Society of Würzburg, and described experiments in which an electroscope was discharged by means of x-rays, and ionization of air was detected. Many observations were made on discharges within tubes filled with hydrogen or other gases. He experimented with different shapes of electrodes, and had already arrived at a forty-five degree angle of the anode as optimum.

The following day he left for a welcome vacation in Italy, but even there found some difficulty in escaping attention as a celebrity. At a party in Como a friend again suggested the name of "Roentgen rays," and despite Roentgen's displeasure the name was adopted more and more widely. The variety of nomenclature proposed during 1896 reflects the extremely wide interest of both the scientifically trained and the laity. The following list includes most of the terms used: skiography, skiagraphy, skatography, shadow print, roentgenography, roentgen picture, radio photography, radiography, electro-skiography, new photography, ixography, electrography, cathodography, fluorography, diagraphy, actinography, pyknoscopy, x-ray photography, x-ray picture, dark light.

Honors came to Roentgen in a flood. A large number arrived during 1896 but for the rest of his life he continued to receive innumerable prizes, medals, diplomas, decorations, honorary degrees, etc. He was made an honorary member of some thirty-four scientific societies and a corresponding or non-resident member of almost as many more. Many of these were scattered throughout the world. Towns hastened to make him an honorary citizen and many streets were named after him. In early 1896, he was awarded the Royal Order of Merit of the Bavarian Crown. He accepted the decoration but declined the privilege of nobility which accompanied it.

Evidence of popular interest continued; comic cartoons appeared in the magazine *Punch* in January and March, 1896, and many similar ones were printed in American magazines. In February, 1896, x-ray proof underclothing was advertised in London. At the same time a New Jersey Assemblyman from Somerset County introduced a bill in the legislature to prohibit the use of x-rays in opera glasses in theaters. Many industrial firms approached Roentgen but he rejected any contract or deal with them and never sought any profit or patent. Thomas A. Edison, who frankly admitted to commercial exploitation of science, commented upon Roentgen's highmindedness but apparently felt that it was rather a matter of naiveté than nobility. Edison, of course, represented the American "go-getter," and was an accomplished showman. He rapidly became the authority on x-rays in the United States, and put on a very impressive public demonstration in New York in May, 1896. But Roentgen's rejection of commercial exploitation was nothing particularly saintly—it was merely the ethos of scientists of his stamp, and was readily understood as normal behaviour by his equals in academic circles. In our time we have the example of Selman Waksman, who has eschewed making any personal profit out of his discovery of streptomycin.

Many off-beat suggestions were raised, such as possibly using x-rays to transmute base metals to gold (here the old alchemical dream re-emerges!). One physicist was approached by a young lady who wanted her boy friend "examined" unaware, in order to check up on his "interior health." It was also suggested that it might be possible by this means to photograph the soul. Equally outlandish was a dead-pan report in *Science* (March, 1896) that x-rays were used to "reflect anatomic diagrams directly into the brains of medical students, making a much more enduring impression than the ordinary methods of learning."

About Roentgen himself there also grew a certain mythology. Several unfounded stories circulated, such as the famous one (which died very hard) of his making his discovery through accidental radiography of a large key.

In May, 1896, the first journal devoted to radiology was published in London, the *Archives of Clinical Skiagraphy*. Even earlier a "Society for Scientific Photography" had been formed in Berlin in January, 1896. Early in 1896, Pupin at Columbia University experimented with fluoroscopic screens within a casette, thus leading to the development of modern intensifying screens. Technical and clinical progress developed fast but these will be considered more fully in a later chapter.

In passing we might notice briefly some economic aspects. Roentgen had, like all department heads, budget troubles, and was always short of money to buy new tubes. Most of them punctured after a certain amount of use. In May, 1896 a Hittorf tube retailed at $2 to $4 in Germany. One legend has it that Roentgen wrote a letter to a manufacturer saying that if he couldn't obtain tubes for the equivalent of $5 he would make them himself. There was some comment also among lay circles that the cost of x-ray equipment might be prohibitive, so that it would never become a significant practical matter in medical work. It was estimated that in 1896 a good radiographic apparatus would cost $300. However, a cut rate portable set-up was offered in St. Louis as early as June, 1896 for $15. At the present time anyone seeking to open an office in radiology can anticipate spending several thousand dollars. Many firms jumped into the market since demand by interested scientists and physicians was high. Very few patents and copyrights appeared since the fundamental principles were common property. The developmental work, however, gave great impetus to many improvements which soon appeared.

During 1896, increasing reports of skin reactions from x-ray exposure appeared and these distressed Roentgen greatly. He had protected himself fairly well, if inadvertently, by constructing a zinc observation chamber, heavily sealed on the side toward the tube. This was designed to exclude any radiation from his experimental procedure beyond that passing through a narrow aperture, but light-tightness for the sake of fluoroscopic observations was probably an equally important consideration to Roentgen. In September, 1896, Joseph Lister commented on the subject of x-ray reactions at a Liverpool meeting of the British Association for the Advancement of Science.

As early as March, 1896, x-rays were used in litigation: the radiographic demonstration of a dislocated bone in the foot of an English burlesque girl settled her claim for damages conclusively. There were quite a few other cases involving radiographic evidence during 1896. The admissibility of such evidence was seriously questioned by several judges, but by 1897 that issue was settled. A case tried in Denver in 1896 (5), includes a particularly eloquent discussion of the merits of x-ray evidence by the judge.

Because of the high degree of interest, Roentgen was solicited by the Prussian Academy of Science to present his next paper to it. He did so in March, 1897, reporting further observations on the properties of x-rays. Most of these were amplifications of detail since the major facts had already been reported. He discussed extensively differences in behaviour of tubes according to the degree of evacuation and used the words soft (i.e., less evacuated) as opposed to hard (i.e., more highly evacuated) to describe the condition of the tubes. These terms have stayed with us to the present day. He also discovered and announced that the x-ray beam is a bundle of rays of differing intensity and penetrability.

The immediately succeeding years saw continued experimental work by Roentgen but he published very little subsequently to compare with the important three papers described. Besides his regular activities he was subject to many harassments and it seems that he reacted excessively to these, particularly to criticism or disparagement. He disliked publicity but enjoyed the honor which he felt was his due, and though shy, reticent and retired most of the time he was capable of great warmth and jollity with his few close friends.

In 1899, an invitation from the University of Leipzig arrived but was refused. However, in April, 1900, a call came from the University of Munich which he felt inclined to accept. It involved the directorship of a new physics institute and considerable prestige. The environment was not quite as comfortable as at Würzburg and working conditions were not ideal. His earlier refusal of nobility from the Bavarian royal family probably led to a less than cordial reception by the court and high society. Relations with the Minister of Education were decidedly stiff—after all, a university professor was a government employee. Nevertheless he settled down and continued to do good work as a teacher although his production of research material declined from this time on. He resumed his previous interest in crystals and occupied himself largely with them thenceforth.

From 1900 on, a very significant shadow lay over Roentgen's life. His wife was afflicted with a chronic illness which caused a great deal of pain and progressed only very slowly. Many restrictions were thus imposed upon most of his activities. Frau Roentgen finally died in 1919, leaving him extremely lonely.

He continued to receive and decline offers of professorships from top ranking universities in Europe as well as from the Carnegie Institution in Washington, D.C. (1902). He was considerably interested in the possibility of a trip to the United States and actually planned one in 1912. This had to be cancelled and it never proved feasible to resume the project. It is worth noting that the numerous offers of professorships received by Roentgen, *even before* his discovery of x-rays, attest to the general recognition of his high caliber. This sort of recognition in academic circles was (and still is) obtained by two principal means: quality of publications, and quality of students trained. A further fact comes out. Roentgen would have been an outstanding physicist of his period even if he had had nothing whatever to do with x-rays.

In 1901, Roentgen was granted the first Nobel prize in physics. He consented to go to Stockholm for the award but declined to give a formal address. The prize money was given to the University of Würzburg, to support scientific research.

In 1905, on the tenth anniversary of the discovery of x-rays, a group of prominent physicists installed a plaque on the Physical Institute of the University of Würzburg stating: "In this building Wilhelm Conrad Roentgen discovered the rays which are called by his name." At this same time a medical group celebrated a special "Roentgen Congress" under the auspices of the Berlin Roentgen Society and founded the German Roentgen Society. Roentgen was made an honorary member.

In 1912, at the Institute of Theoretical Physics associated with his own department at Munich, von Laue, Friedrich and Knipping obtained the first x-ray diffraction patterns. In this some of the theoretical predictions of Helmholtz in 1893 were finally confirmed. One might compare this with the prediction of the existence of the planet Pluto by Lowell fourteen years before its official discovery by Tombaugh in 1930. Although at first skeptical, since he had been unable to perform such an experiment (with much more limited equipment) years previously, Roentgen accepted the finding. The importance of this was to place x-rays definitively in the electromagnetic spectrum. It was the first fundamentally new finding about x-rays since Roentgen's own work.

Roentgen found the World War extremely distressing. He tended to insulate himself in his ivory academic tower and felt very sad that pure science was affected by nationalistic prejudices. He himself displayed a fairly automatic identification with his own country. He was decorated for the contribution of x-rays to the German war effort. The war threw additional academic burdens on him and these, together with his wife's illness, made him overworked and often melancholy.

Although his health had always been good, he had some unexplained lung hemorrhages in 1913 as well as an operation on one ear. He apparently recovered well from these.

During the war his hunting lodge outside the city became more and more a retreat. He grew vegetables and even raised a pig. He took rationing very seriously, and carefully weighed out the permitted amounts of food for the family.

After the war he was guardedly optimistic about the new republic. Most of his investments had gone into German war loans and he lost much of his remaining money in the post-war inflation.

In 1920, he retired from active teaching at the age of seventy-five, though retaining some space in the physics institute. He outlived many of his friends and became more and more lonely. However, he kept up active letter writing with colleagues and family. He tended to dwell more and more on the past, particularly his happy vacations in Italy and Switzerland, and often read over his old citations and honors.

During the winters of 1922 and 1923, he suffered recurring gastrointestinal distress. He became convinced that he had a malignancy of the intestines although no corroboration was forthcoming from his physicians. He grew progressively weaker and finally died on February 10th, 1923. The funeral was extremely elaborate and was made the occasion for many eulogies by prominent scientists.

I have been somewhat struck in reviewing Roentgen's career subsequent to the discovery of x-rays by the apparent decline in productivity. I think that we must keep in mind several facts: one is that we cannot expect a person to produce discoveries of the scope of the x-rays every year, or even every decade. The experimental work done during the twenty years after 1895 was indeed up to his usual standard of meticulous care. His teaching was an important activity and was continued actively and effectively up to the time of his retirement. Finally, one must realize that he was progressively aging and was subject to the heavy burdens of public harassment and his wife's illness. Both of these inevitably sapped his energies.

Still, and most important, I think that we should admire the fact that in 1895, Roentgen was the right man in the right place at the right time—to pick up the opportunity of discovery and bring it to mankind's attention.

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Chapter 3

BECQUEREL AND THE CURIES

WE ARE NOW GOING to meet the founders of radioactivity. As we shall see, Roentgen's work influenced to some extent the investigations which led to the discovery of radioactivity, but this field developed fairly independently until medical applications were found. Thenceforth the history of radioactivity and x-rays remained closely parallel.

In Henri Becquerel we have an example of a discoverer about whom relatively little of a personal nature is known, and whose name is today almost strange to the general public. The contrast with Roentgen (and with the Curies later) consists in the fact that his discovery, though momentous, did not draw comparable public attention and remained for several years in the cloister of the academic world. Without anything to fire the imagination of the newspaper writers and cartoonists, the man's personal life did not get investigated in detail and spread before mankind. In any event, after a few years the major emphasis from the popular standpoint was attracted to the Curies, and they received most of the attention and credit. Nevertheless, it is important to point out that Becquerel preceded the Curies in his discovery, and I have even seen a review of physics since 1890, written by a contemporary American professor, which dwelt on Becquerel but nowhere mentioned the Curies. So widely different is the attitude of the scholar from that of the lay public!

Antoine Henri Becquerel was born in December, 1852 at Paris, into a family already active and prominent in scientific work. Both his grandfather and father were outstanding in research and teaching of physics. The latter was a military engineer with a particular interest in minerals, electricity, and chemistry. He was also a professor of physics from 1837 to 1878 at the Museum of Natural History in Paris. Young Henri's interest and education were early guided into scientific channels, and often through his life he expressed warm affection and gratitude for the early encouragement received in his family.

Becquerel studied at the Polytechnic School in Paris, proving to be a good scholar, an indefatigable worker, and very apt in physics. His first scientific article appeared at the age of twenty-two, one year after graduating from the Polytechnic School. His education was not confined to science, and he was evidently an all-round cultured person with educated tastes in the arts and humanities. All of the adjectives applied to him bespeak a man well adjusted to the world, simple and unassuming but with considerable personal charm and savoir faire in the art of putting himself across. This is merely emphasized to contrast in minor degree with Roentgen's shyness and strong reaction to public attention; but in considerably greater degree to the personal dismay and distress of the Curies when subjected to popular acclaim.

In 1875, Becquerel entered government service in the department of roads and bridges, eventually becoming Chief Engineer. In 1892, he succeeded to his father's chair of physics at the Museum of Natural History, and in 1895, he became professor of physics at the Polytechnic School. All of this time he was active in many research projects as well as carrying on his teaching schedule. His fundamental interest was to investigate the nature of matter, and this was approached through the properties of elements. He did much work on magnetism, polarization in crystals, properties of the rare earths, and phosphorescence. In this variety of interests over the years we are reminded of Roentgen's career, and like Roentgen he appears to have been good both in experiments originating from a theoretical basis and in noting significant deviations and new phenomena.

His interest in phosphorescence had been a long continuing one, and indeed his father had also worked on this property of uranium. Becquerel's attention to further research on uranium was specifically attracted by an observation of Henri Poincaré in 1896. The latter, in commenting on Roentgen's discovery, wondered if naturally fluorescent minerals might emit any rays similar to x-rays under the influence of light. Such ideas are the building blocks of research, and Becquerel was studying salts of uranium when he detected their spontaneous emission of rays in the absence of light. The story is told that a sample of uranium in sunlight was being studied as to the degree of development of a shielded photographic plate under it. In one of these experiments the sun went behind a cloud and the day grew gloomy. The test was interrupted and the cassette put aside. But when it was processed a day or two later, the development of the emulsion was equally strong, even without bright sunlight upon the specimen. Here Becquerel exhibited the significant acumen of the gifted man, by recognizing the finding as important and worthy of further investigation.

Another story is told of his discovery of one biological effect of radioactivity. He was carrying some 200 mg. of uranium in a tube in his vest pocket for several hours and found a burn upon the skin underneath it. Again the creative spark was there and he correlated the skin erythema with the mineral. The skin later underwent ulceration, and took an extremely long time to heal. This occurred in 1901, and it was almost the first biological effect of radiation noted in a human. The very first experiments on human skin had been performed by Walkhoff and Giesel in Germany, in 1900.

It may be worthy of note that Becquerel's personal life and professional activities were much less restricted through poverty and lack of space than was the case with the Curies subsequently. His brilliant performance as a student and early rise up the academic ladder gave him an assured position of authority and appropriate working conditions. Nevertheless, physical facilities for scientific research in France must have been not too good in the 1890's and early Twentieth Century since many foreign observers commented on the rather niggardly budgets and space available to Becquerel and of course, much more so in the case of the Curies. These were contrasted with much better facilities available to persons of comparable standing in such countries as England, Germany, and the United States.

The announcement of his discovery of radioactivity was presented to the Paris Academy of Science in November, 1896. The name "radioactivity" was only coined later by the Curies and for quite a few years the emanations of uranium were known as Becquerel rays. He continued active work in follow-up experiments and over the next few years much of this was conducted in collaboration with the Curies. The high point of his career was given signal recognition by the joint award of the Nobel prize for physics in 1903 to Becquerel and the Curies.

He continued further teaching and research uneventfully and died in 1908 at the rather early age of fifty-six.

The lives of Pierre and Marie Curie are rather better documented. Their discoveries bore practical fruit within their lifetimes and this apparently is what makes the vast difference in the degree of publicity.

Marya Sklodowska was born in November, 1867, at Warsaw. Her family was one which particularly favored intellectual growth, both parents being well educated and school teachers. The level of intellectual performance in this family was far above the average, and for this reason Marya, distinctly precocious, did not stand out too strikingly above her brother and sisters. She learned to read at four, and early demonstrated an extraordinary memory.

Economically, life was difficult, due to the heavy schedules and poor pay available to teachers. Teaching was severely supervised by the Russian government, and it was only with great difficulty that anything relating to Poland as a nation was communicated to children. Naturally this led to conspiratorial efforts on the part of all concerned to preserve and perpetuate a national consciousness. Poland was always simmering on the verge of rebellion during the nineteenth century, and all young intellectuals identified themselves passionately with patriotism.

Marya's mother died of tuberculosis during her childhood, and there is some question of her having caught the disease and suffering from it later. She was never willing to accept any sort of rest cure, and if she did have tuberculosis, she certainly demonstrated her inherent toughness by shaking it off.

Marya finished the gymnasium in June, 1883, at the age of sixteen, having done extremely well in her studies. The universities in Poland were closed to women, so no possibility of higher education existed there. All Poles hated the occupying powers of

Russia and Germany, so that the eyes of any who aspired to advanced study went directly to France. Besides, the image of France as the land of liberty was firmly set in European consciousness since the revolution of 1790.

However, no definite plans were made at this time, and further education was just a dream. Marya took a one year holiday, visiting relatives in rural districts in several parts of Poland and had a wonderful time just resting. Her family had many recent roots in the country, and Marya had a strong feeling for the soil. She returned to Warsaw in September, 1884, and began to help out at home where her brother and two sisters were already assisting their father by tutoring students while continuing their own studies. Marya began to find science more and more interesting. Her father had been a teacher of physics, and she had been fascinated as a young child by some of his apparatus in the family living room. Now, as an adolescent, she was caught up in the intellectual ferment which was idealizing the exact sciences as the road to wealth, power, and independence for Poland. Marya's political views, if any, tended toward what we would now call mild socialism, but she was much too shy of regimentation to join any formal party, and throughout her life shunned Marxism particularly, perhaps because of its identification with Russia.

In 1885, she came to an important decision. She made a pact with her older sister, Bronya, whereby the latter would go to Paris and study while Marya would work as a governess in Poland and help to support her. The plan was that when Bronya finished her studies for the doctorate in medicine, she would return home and help support Marya at Paris. This romantic and adventurous plan reminds us of how the singers Rosa and Carmella Ponselle did essentially the same thing in taking turns at studying.

The plan was put into effect, and for the next six years Marya had a succession of jobs in country houses tutoring children and working as a governess. Over this period she gradually despaired of being able to further her own education. She transferred her ambitions to her brother, Joseph, who was following a medical course in Warsaw, and to her sister Bronya who was doing the same in Paris. During this period she also had a desultory love affair which died a gradual death, and this, together with a fading of her original Catholic faith, helped to accentuate her gloom. While working, she tried to continue studying on her own out of books, but without teachers or laboratory equipment she could make only limited progress in such subjects as chemistry, which particularly interested her.

Finally, in 1890, Bronya married another expatriate Pole in Paris and felt the time had come to take the plunge of inviting her sister. She did so, with the understanding that Marya could save money by living with her for the first year. By this time Marya was so discouraged with her own prospects that she declined the opportunity. However, her sister would not take "no" for an answer, and without too great difficulty, got Marya's consent. It was finally arranged that she would stay in Warsaw and work for one more year before going to Paris.

The contacts with educated people in Warsaw brought her back to life intellectually, and for the first time she had the opportunity to perform simple experiments in a chemistry laboratory. Her ambition took fire again.

In September, 1891, aged twenty-four, she was ready to go. In November of that year she started her courses at the Sorbonne. Her life was happy and hard working, but of course very restricted economically. Living with her brother-in-law and sister, she was exposed to a considerable amount of social life among Poles who were studying or exiled. She met Paderewski and maintained a life-long friendship with him. In early 1892, however, she found it necessary—in order to have some peace and privacy for work—to move away to lodgings of her own, nearer to the university. This meant paying for food and rent, and she managed to survive on approximately 100 francs ($25) a month. Everything had to be cut to the bone. She ate poorly, lost weight, and grew anemic.

Scholastically, she did well and received the master's degree in physics in 1893. In 1894, she received another master's degree in mathematics. One can see her ambition progressively growing by what it fed on. In the summer of 1893, she made a visit to Poland with somewhat uncertain plans. She yearned to return to Paris for further study, but had no specific end in view and no resources.

Some friends procured for her the Alexandrowitch scholarship of 600 rubles. With this fairly generous stipend, she returned to Paris. Years later, she scraped to repay the scholarship money out of her earliest earnings. This was the only recorded instance of such repayment in the history of the fund.

In 1894, Marya, then twenty-seven years old, accepted a project on magnetic properties of various steels commissioned by the Society for Encouragement of National Industries in France. It was in the course of preparing for this investigation that she was introduced to Pierre Curie, then thirty-five years of age.

Pierre had been born in Paris, on May 15th, 1859, the son of an Alsatian Protestant physician. His education had been informal and private but intensive, and the intellectual atmosphere was not unlike that which had prevailed in Marya's home during her childhood. Pierre also demonstrated precocious scientific talent, and at the age of sixteen received his bachelor's degree. He received a master's in physics at eighteen. He and his brother, Jacques, also extremely talented, worked together at the Sorbonne and developed extremely valuable work on "piezo-electricity." Like the other famous figures we have discussed in the last chapter and this one, Pierre would have been remembered as an outstanding scientist even without any involvement in radioactivity. He was already famous in scientific circles, and in the early 1880's, Lord Kelvin, on a visit from England, sought him out particularly. Pierre was almost a recluse toward the world, and his only enthusiasm was science. He declined advancement and honors with what seems excessive compulsion. He did this even though it meant continuing to work on drudgery and in poor surroundings. A disappointing youthful love affair may have had something to do with making him renounce everything worldly.

Both Marya and Pierre recognized kindred spirits early in their acquaintance. As they became better acquainted in scientific fields, they also became charmed personally. Almost the only thing which Pierre did not admire about Marya was her devotion to Polish patriotism, for in his almost monastic view, even this seemed to detract from dedication to science. In June, 1894, Pierre received his doctorate with a brilliant thesis. He proposed to Marya at the same time, but she was quite uncertain. She felt confused as to whether she might be betraying her homeland by abandoning active work for Polish liberation. It took one and one half years of persistant courtship by Pierre to wear down her scruples, but finally they were married, very simply, in July, 1895. The honeymoon consisted of a bicycle trip in the French countryside, and this remained one of their chief hobbies for many years.

Marya had always shied away from domestic economy, but now she felt obliged to learn how to keep house and cook. Even so, the ménage was Spartan in the extreme; both Pierre and Marie continued active in laboratory work and teaching. Their social life was confined to a few intimate friends and family. In the summer of 1896 Marie passed her fellowship examination with high honor. In September, 1897, their first child, Irène, was born. There was no thought by either Pierre or Marie of dropping a scientific career for the family. Care of the child was carefully organized and she continued her work.

The next academic step for Marie to consider was whether or not to work for a doctor's degree. She cast around in search of a thesis subject, and during 1897 lit upon Henri Becquerel's 1896 report on radioactive emanations from uranium. This took her imagination, and having been invited by Becquerel to pursue the subject, she set about active work. The quarters available were hardly to be called a laboratory. A dark, damp, unheated storeroom with no benches or electrical fixtures was all that could be found. Nevertheless, she made do and set about the first investigations. Early in the work she coined the term "radioactivity" which has remained in use to date. The project expanded rapidly, and in April, 1898, she gave a preliminary communication to the Academy, announcing the probability of a new element in pitchblende endowed with powerful radioactivity.

Pierre was observing this work from a distance, but was busy with his own responsibilities. In June, 1898, however, he became so interested that he dropped his own work and entered fully into partnership with Marie. For the next eight years they worked closely together and everything was done jointly. They assiduously avoided any individual credit. By July, 1898, they suspected at least two new elements, the first identified with certainty, and in July the name *polonium* was proposed. In December, the second new element in pitchblende was announced and the name *radium* suggested.

In order finally to convince all skeptics it was necessary to isolate and weigh these new elements. This took the next four years and incalulable hard work. Financial questions also arose, such as how to pay for the large amount of ore which must be extracted (since the elements were only present in tiny traces). They succeeded finally in obtaining from the Austrian government a gift of one ton of slag from the uranium mines at Joachimsthal. A few years later, Austria realized how valuable uranium was, and clamped on an embargo.

Working quarters were another problem: they finally accepted the only available place, one nobody else wanted. It was a leaky shed at the School of Physics. Here laboratory conditions were almost inconceivably bad, and it is tragic to think of the amount of labor wasted through poor equipment. Much of the extraction work had to be done out of doors in a court yard since there was no inside chimney. The dirt, drafts, cold, smoke, heat and miserable equipment all became more and more of a burden as the years passed. Finally, in 1902, a small amount of pure radium salt was weighed and registered with its atomic weight of 226. Radium officially existed.

Throughout the period of work on refinement of radium, conditions of life had been difficult at home as well as at work. In order to supplement income, Pierre had taken a tutoring job which involved many hours of dull routine. The possibility of a professorship at the Sorbonne was repeatedly postponed because of professional jealousies, and Pierre's inability to blow his own trumpet. Only in 1904 was he granted an appropriate recognition as professor. Marie took a teaching job in physics at a high school for girls outside of Paris. These hours of work took time and energy away from their research project. During this period also, there was real alarm over the possibility of Marie having active tuberculosis, but she refused any treatment, and regained her health gradually. Faithful friends tried to advance Pierre by getting him into the Academy of Science and to accept the decoration of the Legion of Honor. The latter he refused, even though it has always been commonly accepted as a mark of distinction in France. In this way he antagonized some people, just as had Roentgen by declining to add the prefix "von" to his name. As to the Academy of Science, there appears to be a standard pattern in France of having at least two candidates for any one professorship or vacant position in the Academy; inevitably this produces competition and invidious hostilities. Pierre was constitutionally unable to participate in the necessary politicking and inevitably lost out. Also in 1903, Marie had lost a pregnancy, probably because of her generally rundown condition. This caused her great distress, and her father's death at about the same time added to her melancholy. Conditions at work exacerbated a recurring rheumatism in Pierre, and seriously disabled him from time to time.

Nevertheless, some thirty-two scientific papers were published by the pair between 1899 and 1904, mostly together, sometimes alone, and sometimes with a collaborator. A few scientists had been attracted by the early findings of radioactivity, and were conducting comparable experiments, both in Paris and elsewhere. One of these, of course, was Henri Becquerel. Another was Schmidt in Germany, who discovered radioactivity in thorium while Marie's work was in progress. In 1901, shortly after the reports of Walkhoff and Giesel, and after Becquerel got his skin ulcer, Pierre deliberately tested the effect of radium on his own arm, and developed a nasty lesion. In 1904, he participated with two physicians in experiments with radium on animals. They noted that radium killed diseased cells preferentially, and hence the idea of using radium on tumors originated. Here was the first suggestion of a practical use for radium and suddenly a commercial interest in its development appeared. We will investigate later the commercial and therapeutic exploitation of radium.

In 1903, Marie had finally found time to assemble the material for her doctorate thesis and the degree was awarded with exceptional honorable mention (très honorable). Actually, the research had far outgrown the dimensions of the thesis originally envisioned and had become a large scale project in its own right.

Recognition, though late, now began to accumulate. In 1903, Pierre was invited to London to lecture before the Royal Institution, by invitation of Lord Kelvin. The Curies' circle of scientific acquaintances was thus enlarged, and the expedition was very successful. The Davy medal was awarded to Pierre later in the same year. In December of 1903, the Nobel prize for physics was jointly awarded to Henri Becquerel and to Pierre and Marie Curie. They did not attend because of poor health, but the prize money was extremely welcome, and was gratefully accepted since it was an honorable trophy with no strings attached. At last it permitted a slightly greater degree of comfort than they had had before. Most of the money was put into securities after a few benefactions had been made. However, Marie continued to teach and permitted only a few home improvements, such as necessary plumbing, and new wall paper in one room. She never thought to get anything for herself... even a hat.

Public attention now focused upon the Curies because of the Nobel award and the growing medical uses of radium. The harrassment of their personal life was even more distressing to them than it had been to Roentgen. They fled from it, reacting with horror. To them peace and the opportunity to work undisturbed were the most important values. Now these were almost impossible, and in their already fatigued state, their nerves were badly frayed.

At home in France, academic circles suddenly became aware of the celebrities in their midst, and a chair of physics at the Sorbonne was finally awarded to Pierre. Pierre had already refused a few years previously the offer of a professorship and generous facilities at the University of Geneva. Even though the offer had looked very good, and he had made a preliminary acceptance, he declined finally, because he felt that it would interrupt the work on radium.

In December, 1904, Marie's second daughter, Eve, was born, and in convalescing from this she regained most of her strength. In June, 1905, the Curies finally went to Stockholm to fulfill their obligation to give a Nobel lecture, and had a fairly pleasant trip without too much public furor.

During this time Pierre and Marie allowed themselves occasional lapses from the grindstone, and took in a few plays and concerts. They met and formed acquaintances of varying warmth with such celebrities as Eleanore Duse, Sully, Sarah Bernhardt, Lucien Guitry, and Auguste Rodin. They even had a passing interest in spiritualistic séances, but were naturally disappointed by the lack of scientific basis and the inevitable exposure of hoaxes. Spiritualism is such an unlikely interest for the rigorous scientist! One is particularly surprised that two such thorough pragmatists had even the slightest flicker of interest. Perhaps their lives, vacant of religion, craved a little of the mysticism that most of us need.

In 1905, with fame established, it became socially and academically necessary for Pierre to enter the Academy of Science. Again he went through the routine, distasteful to him, of paying ceremonial calls and engaging in competition against another candidate. In July, 1905, he finally was elected, but by a close margin, and he never overcame his disgust with the Academy. For those who could play the game the Academy was a useful forum, and much excellent work was done by members. Prestige of old fogies was protected, of course. The system may seem unnecessarily rigid to us, but may not be too different from senatorial seniority in this country. Plus ça change, plus c'est la même chose.

Despite official recognition Pierre was disappointed with the niggling official attitude on perquisites of his Sorbonne professorship. No laboratory space at all was given him at first, but through persistent nagging some half-way acceptable quarters were finally made available.

In April, 1906, Pierre's career was abruptly cut short at the age of forty-seven by a tragic accident. While walking along a rainy street, preoccupied in thought, he stepped into the gutter at a crossing and right into the path of a heavy dray. Before anything could be done, he was trampled by the horses.

The death of Pierre was utterly shattering to Marie, and it took her some time to pull herself together. Reaction on the part of their friends, the public, and the government was extremely warm, but gave her little comfort. A government pension was offered to Marie, but was refused. However, through the good offices of friends her husband's chair of physics was awarded to her in May, 1906. This was the first such appointment of a woman in the history of the Sorbonne.

At her first lecture in November, a large and curious crowd awaited her in the auditorium. Marie visited Pierre's grave before going to the university.

Tradition demands that a new professor commence his career with a eulogy of his predecessor, and compliments to contemporary academic figures, as well as the Minister of Education. But Marie had no use for such polite formalities. Without a break she took up the lecture at the precise point where Pierre had left off in his course the previous April.

Gradually she resumed a regular life with her academic duties, bringing up the children, and care of Pierre's aging father. She dreamed of founding a laboratory worthy of Pierre's and her ambition to train scientists in concentrated research on radioactivity. With all of her responsibilities, she gave much attention to her children and to nursing old Dr. Curie until his death in 1910.

She had no desire for private wealth, nor to secure the financial independence of her children. She felt it quite normal that they should earn their living like anyone else. Her private gram of radium was given away as a matter of course. Radium at this time was worth about $150,000 per gram. As early as 1900, Pierre had made her aware of the possible wealth to be obtained by exploiting their discovery, but both rejected without reservation any idea of private gain, even though, in the harsh present, it meant giving up comfort, good laboratories, and adequate assistance. Anyone who asked them for details of methods received a full answer.

During this time she initiated the first, and for some time the only, course on radioactivity in the world. In 1910, she published a book of 971 pages on radioactivity. In her own laboratory she undertook to purify radium metal, a tour de force, since there was no practical use for the pure metal, and this project has never been repeated. She devised a means of measuring the amount of radium by its emanation, since amounts used and available were so extremely small that balances were of no use. She prepared the official radium standard deposited in the bureau of weights and measures at Sèvres.

In 1910, Marie was offered the Legion of Honor, which she refused, as had Pierre. She bowed to the advice of friends to present herself to the Academy, but was even more dismayed than Pierre by the controversy aroused by partisans of herself and the other candidate, Branly, who was also a very good scientist. Much bitterness was aroused, and Marie was quite incapable of playing the game by traditional rules. By her diffidence she hurt her own cause, and she missed the election by one vote. This did nothing to enhance the prestige of the Academy either in her eyes or in those of the world.

In December, 1911, her second Nobel prize, this time in chemistry, was awarded. This is the only instance of any one individual receiving more than one Nobel award.

Despite general acclaim and growth of a legend about her, hostility arose in some quarters, and an attempt was made to link her scandalously with another scientific colleague in Paris. She was harried by newspapers which accused her of being a foreigner (e.g., Russian, German, Jew) when they didn't like her, and identified her with France when she achieved something important. Anonymous letters and threats came, and all of this meanness merely drove Marie more in upon herself as a recluse. In 1912, with her health and nerves badly taxed, she suffered a serious illness and required an operation on one kidney.

In this year came an offer of directorship of a radioactivity laboratory in Warsaw, and naturally she was greatly touched and stirred by this voice from the homeland. However, a new laboratory for radioactivity had been started in Paris in 1909, and she felt that she could not give it up since it embodied the dream she and Pierre had shared. Torn between the two strong pulls, she accepted an advisory role at Warsaw but arranged for the active directorship to go to a man whom she knew to be competent.

In 1913, Marie's health was better, and at this time she became acquainted with Albert Einstein.

In July, 1914, the Radium Institute was finally finished, culminating many long years of work. It was planned as a dual project with a research branch under Marie and a biological and medical side under Claude Régaud.

All normal activity was suspended, however, in August, 1914, by the war. Marie at once sought where she might be of most use. The government was much too busy to give her any consideration, so that she had to work pretty much on her own. She found that the French military medical service was almost devoid of x-ray equipment. It is rather amazing that nineteen years after the discovery of x-rays and their world-wide acceptance, the military service of a major power had not availed itself of this tool. Marie saw the need clearly and moved into high gear to do something about it. Her first project was to develop a mobile radiologic unit and this materialized as a medium truck with power generated by the motor. She had to scrounge money, equipment, and material, and gradually refined to a high degree the art of worming through red tape to get something done. The French had a word for it: "System D." She actually developed twenty mobile radiologic units during the war and these circulated actively between military hospitals. With these in motion, she also set up two hundred fixed x-ray installations for the army. Everything had to be done from zero. Manufacturers of x-ray equipment had to be nagged and hounded. Technicians had to be trained—after she had trained herself to be a good technician. Between 1916 and 1918 she trained at least one hundred and fifty technicians and besides gave classes to American soldiers. The x-ray equipment of the American army (and incidentally the German) was considerably better than that of the French. Indeed, as early as 1897 the British army had adapted portable x-ray units to field conditions. From all this we can see that Marie Curie takes her place among pioneer roentgenologists since she effectively practiced the art in collaboration with the military surgeons.

After the war Marie was very happy at the rebirth of the Polish nation. She faced the future with her health exhausted, her work completely interrupted for four years, and with no resources except her salary of twelve thousand francs a year . . . all of her money, including Nobel awards, had been ploughed into French war loans.

Characteristically, she dived into active research at her institute, and her daughter, Irène, was gradually drawn in as collaborator.

In 1920, Marie was visited by a Mrs. Meloney from America who interviewed her for an American magazine. Mrs. Melony was greatly struck by Marie and devised a scheme to buy her a gram of radium by subscription in the United States as a gift and token of American esteem. In return, of course, Marie had to visit the United States. This was turned into a triumphal tour with honors, adulation, academic degrees and resultant embarrassment. President Harding made the ceremonial presentation of the gram of radium (only a dummy was used at the ceremony) and Marie insisted, immediately upon transfer of the gift, on taking legal steps to make it the property of the Radium Institute rather than herself.

The trip was not without its difficulties. Marie's right hand was injured by the violent shake of a well-wisher and her strength collapsed entirely before the tour was half over. It became necessary to husband her strength and streamline everything for the remainder of the visit. Occasionally her daughters appeared for her. Newspapermen badgered her to extract an opinion about the forthcoming boxing bout between the Frenchman, Georges Charpentier, with Jack Dempsey. Poor Marie was hardly conscious of an event so foreign to her world!

On the good side, Marie was strongly impressed with the calibre of American universities. She was pleased with the extensive application of radium in treatment. She liked her American hosts in general and formed many warm attachments.

Inevitably, with exposure to America, she was made to think again about whether she and Pierre would have been better off if they had taken out patents and exploited radium for themselves. She again rejected the idea, saying in effect that the world needs dreamers who require no material profit and don't want it; *but* (like all such dreamers) she felt that the world ought to take care of them.

During the 1920's, she continued active work, much of it now concerned with teaching and direction of students and associates at the Institute. Like all prominent figures, she received many importunities for support of various causes, but the only one she allowed to claim her attention was the League of Nations. In 1922, this body named her a member of the International Commission on Intellectual Cooperation. Characteristically unable to give only token support, she dived into active promotion with many other contemporary notables such as Bergson, Gilbert Murray, and Einstein. She worked productively on such projects as an international coordination of scientific bibliography, unification of terminology and symbols, and format of scientific publications. The committee further sought coordination of research on an international level throughout Europe. She even, contrary to her own personal philosophy, lent support to obtaining copyright protection to give incentive to impoverished scientists.

In 1922, she was finally elected to the Academy of Science by acclamation. In 1929, she returned to the United States a second time to accept another gift gram of radium. This time it was intended for the new Warsaw Radium Institute, which was formally inaugurated by Marie in 1932.

During her last years she was made happy by the marriage of her daughter Irène to Frédéric Joliot, her most brilliant pupil at the Radium Institute. This marriage took place in 1926, and when the Joliots' child, Hélène, was born, Marie became a very affectionate grandmother.

Although her health had remained generally good, Marie developed cataracts after 1920 and these were extracted in 1923. Her hands were much damaged by radium and finally, between 1933 and 1934, she fell ill with obscure symptoms. She finally died in July, 1934, at Paris, of aplastic anemia, which may logically be attributed to her prolonged exposure to radiation. She was sixty-seven years of age.

We have now reviewed the careers of the founders of radiology—all physicists. From now on we will turn our attention to the practical applications of what they set in motion.

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PART II

THE DEVELOPMENT OF RADIOLOGY

Chapter 4

PROGRESS SINCE 1895, THEORETICAL AND TECHNICAL

In surveying the development of radiology since 1895 one is faced with a bewildering mass of data—immense in volume, widely varied in scope. How to go about organizing it intelligibly has been a real problem.

Although a simple "horizontal" approach, taking year by year like layers of cake, is a fairly obvious way to do it, I am afraid that it is too easy in that way to lose the thread of progress in any one subdivision. So I have adopted rather a "vertical" method of tracing chronologically the growth of a number of individual fields. I think that the inter-relationships will emerge as we go along.

PHYSICS

Probably in every age people can be found who will cheerfully go out on a limb to make broad generalizing statements which look silly to observers 50 or 100 years later. A fine example of this (3) is a comment by the prominent American physicist Professor Albert Michelson (who did valuable work in his own right). He made the following pronouncement to a scientific gathering in 1895: "While it is never safe to affirm that the future of physical science has no marvels in store, it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles."

Perhaps this statement is not truly representative of most physicists at the end of the nineteenth century, but coming from such a source, it surely indicates a fairly wide segment of opinion. In extenuation of Michelson it has indeed been suggested that at that time atomic theory was largely in the domain of the chemists, and thus might seem to him to have little or no importance in physics.

Nevertheless, even this apology indicates a somewhat restricted view.

The very fact that the surface of the physical pond was so free of ripples made Roentgen's announcement of the "new kind of rays" particularly disturbing. Physicists all over the world reacted with a furious spurt of experimental activity. One contemporary wrote, early in 1896, that the discovery "stimulated hosts of inquirers and imitators into a scientific scramble, the like of which has probably never before been seen." He continued prophetically, "it is probable that much of the final profit will be derived from such other interests as grow out of it" (4).

The easy reproducibility of Roentgen's experiments secured rapid acceptance of his findings, even though a few—like Lenard—clung for several years to the idea that these rays were just cathode rays, with some new properties discovered.

For quite some time, however, acceptance and understanding were far apart. The prevailing opinion during the first few years was that these new rays were related to the electromagnetic spectrum and the scientists gropingly sought to find out more. The situation was succinctly stated by the Englishman, A. C. Swinton: "It reminds me of the old definition of metaphysics—a fellow talking about what he doesn't understand to another who doesn't understand him" (2).

In any event, experimental work on properties of x-rays was rapidly undertaken and extended in physical laboratories everywhere. For instance, ionization of air was recognized and studied in 1896 and was already suggested as a measure of quantity by Perrin. Scatter radiation was also recognized early and gave rise to much debate as to its nature; reflection or refraction of the primary beam was thought probable by many.

The next big landmark after Roentgen's discovery was Becquerel's report of natural radioactivity in November 1898. This was of course greatly amplified by the work of the Curies during the ensuing years.

In 1897, came the first indentification of the electron by J. J. Thomson, based on study of cathode rays. Coupled with the measurement of the charge of the electron by Millikan in 1909, we have the culmination of some fifty years of work starting with Faraday and electrolysis. A common constituent of atoms of all elements was definitely established, bringing forward by a significant step the idea of atomic structure.

In 1897, Ernest Rutherford identified alpha and beta rays emanating from uranium and in subsequent work he correctly identified them as helium nuclei and electrons respectively.

1898, Villard recognized gamma rays and observed their similarities to the Roentgen ray.

In 1901, Max Planck proposed his famous quantum theory, according to which radiant energy behaves as discontinuous particles. We are still, of course, trying to sort out the wave theory and the quantum theory, and you are all familiar with the frivolous suggestion that light consists of waves on Mondays, Wednesdays and Fridays, and of corpuscles on Tuesdays, Thursdays and Saturdays. Presumably on Sunday physics takes a rest, and perhaps doesn't care.

In 1905, Albert Einstein proposed his famous equation $E = mc^2$, stating the relationship of energy to mass.

In 1910, Soddy suggested an explanation for atoms with slightly varying weights but identical chemical properties, and called these isotopes. A wealth of further experimental work firmly established the natural occurrence of stable isotopes long before the radioactive kind became available.

On the basis of work done during the previous decade, Rutherford in 1911 proposed an atomic theory with a distribution of mass and charge essentially that which we accept today.

In 1911, Hess discovered cosmic rays.

In 1912, von Laue, Friedrich, and Knipping discovered and proved the diffraction of Roentgen rays by crystals, thereby firmly establishing their relation to the electromagnetic spectrum, and confirming theoretical predictions made by Helmholtz before 1895.

In 1913, Niels Bohr suggested an atomic structure involving a central nucleus with orbital electrons in layers around it.

In 1919, Rutherford bombarded nitrogen atoms with alpha particles and observed the production of hydrogen and oxygen. The hydrogen ions were called by him protons. Here is one of the major milestones, the first controlled experiment in which one element was artificially transformed into another. In further work Rutherford and others demonstrated and identified the neutron. Discovery of the deuteron must be credited to Harold C. Urey, who discovered heavy hydrogen in 1932. Aside from its theoretical interest, the deuteron was used extensively as a projectile in subsequent experiments bombarding various atoms.

In 1932, Ernest Lawrence (who died in September, 1958, after a long and fruitful career at the University of California) invented the cyclotron. This, as you know, is a chamber in which it is possible to accelerate particles to immense speeds for use as projectiles. It is related that at the very outset Lawrence demonstrated the feasibility of the cyclotron by putting together $25 worth of miscellaneous metal and wood taken from a kitchen chair, a clothes tree, a four inch electromagnet, and a vacuum chamber made of glass, brass, and sealing wax.

In 1934, Frédéric Joliot and his wife, Irène Curie, produced artificial radioactivity by bombarding aluminum with alpha particles. During the next ten years all of the known elements were bombarded in one way or another to produce radioactive isotopes.

In January, 1939, Hahn and Strassmann, in Germany, reported fission of uranium-235 when bombarded with neutrons. They and others realized that in this splitting large amounts of energy must be released. It is important to emphasize that this energy release was the first practical demonstration of Einstein's theoretical prediction of 1905.

In 1940, D. W. Kerst constructed a betatron in which electrons were accelerated to energies of twenty million volts, and later up to 300 million volts, by magnetic induction.

We are now approaching the last chapter of the development of physics. And although I originally set out to keep this section on a starkly technical plane, that is just not possible. Physics became everybody's business when it found how to release for practical uses the forces that keep sub-atomic particles in normal relation.

Many scientists could see that it was only a matter of time until the lessons in "atom-smashing" begun with Rutherford in 1919, and intensified with cyclotrons after 1932, bore fruit in harnessing atomic power.

In January, 1939, at the same time that Hahn and Strassmann's pioneer work on uranium fission was published, Niels Bohr was visiting in the United States. He discussed this new development with several physicists at Princeton and Columbia. The possibility of neutron emission during the fission process was at that time considered by Enrico Fermi (who was working at Columbia), and although this was then only a guess, he went further to suggest that it might permit a chain reaction.

Work on fission was undertaken everywhere, and a review article in December, 1939, covered some hundred papers on the subject. Native Americans were perhaps naive. It was the fairly numerous group of foreign-born physicists in this country (Wigner, Szilard, Teller, Fermi, and others) who, seeing the war clouds gather, knew what might happen in Europe. They took the iniative, as early as the spring of 1939, to apply a voluntary censorship to publication of work on uranium. This suppression—with notable lack of cooperation by Joliot—was indeed effective by 1940, on an entirely voluntary basis throughout the war, and applied equally to foreign physicists outside the "axis" countries.

These men knew that alert scientists and officials in Germany would see the military possibilities of atomic power. Sure enough, by early 1940, word leaked out that an intensive research effort was being made in Germany. It requires little imagination to see how different might have been the outcome of World War II if Hitler had obtained control of atomic bombs first.

In March, 1939, Fermi and Pegram from Columbia contacted our Navy Department. They were interested, but took no action. By fall several other scientists, with the interested backing of Albert Einstein, contacted President Roosevelt directly. He was impressed, and appointed a committee to investigate and make recommendations. The result of this is detailed in Professor H. D. Smyth's famous book *Atomic Energy for Military Purposes* (5). With that title, and coming from a physics professor, you might expect a rather dry account, but most of the book reads like an adventure story, and it is a "must" for any educated person of our generation. Another fascinating book about the same period is *Men and Atoms* (6) by W. L. Laurence, science editor of the New York Times. He was privileged to be the official reporter of the bomb development project, and gives a wealth of interesting detail.

Within a year some $300,000 of government money had been ploughed into research, and the progress was sufficiently encouraging to arouse real hope of making atomic power available in a reasonable time. By means of pumping in big money and organizing most of the nation's physicists in a unified program the speed of development was suddenly and sharply accelerated.

The crucial break-through was the successful initiation of the first self-maintaining nuclear chain reaction in a uranium pile by Enrico Fermi at the University of Chicago on December 2, 1942.

From there on about three years of feverish secret work were required to iron out the practical details and put the theory into hardware.

It is amusing for me to remember now that while I was a medical student one of my many odd jobs was that of a night watchman in a physics laboratory in Havemeyer Hall at Columbia College early in 1943. There I kept flow-rates constant and took hourly readings between midnight and 8:00 A.M. I used to sit on a window sill smoking my pipe until someone pointed out to me a waste hydrogen exhaust pipe right next to my seat. One night, while having a few beers with one of the young physicists before going on duty, this fellow confided to me very guardedly, and half incredulously, that he *thought* the work in our laboratory had something to do in a vague way with release of nuclear power, and that this might result in a terrifically powerful bomb. I didn't take him seriously.

This period of practical application is not so important to the history of theoretical physics as the landmarks I have been emphasizing. But one story (7) from that time I feel deserves immortality, and I want to pass it on to you. It gave me a tremendous thrill when I first heard it.

A young physicist named Louis Slotin, working at Los Alamos in May, 1946, was observing the pile and was horrified to see that the chain reaction was going critical too fast, probably because the control rods (for some unexplained reason) were not operating properly. He realized that if nothing were done there would be an explosion that would blow up the whole area. The only thing to do was break up the pile. And this he did, with his hands, fully recognizing that he was exposing himself to an amount of radiation which would probably kill him. It did kill him within a few weeks, and his was one of the first cases of acute overwhelming radiation exposure available to medical observation. This man was truly a martyr.

The practical results of the 1942 chain reaction were, immediately, the atomic bombs which were exploded on July 16th, 1945, in New Mexico, on August 6th, in Hiroshima, and on August 11th, in Nagasaki. Refinements of the military uses of atomic energy have been considerable since, of course, and we now have some hope of a "clean" bomb which will not increase the world's burden of background radioactivity. On the more positive side, isotopes for research and medical use became available in 1945, and the spectacular spread of their usefulness will have to be the subject of a subsequent chapter. Much progress has also been made in a short time in applying nuclear power to production of practical mechanical work. We all know about the nuclear powered submarines which are functioning very successfully. I have read a very interesting discussion of their operation (8), and it was there emphasized that the sailors on the Nautilus, except for a few engineering personnel, get actually less radiation than shore based sailors or we landlubbers—structural protection of the reactor is really good, and there is less background radiation at sea than on land.

But, while a military submarine is not subject to the economic pressures of private business, it does not seem unreasonable to hope that nuclear reactions will eventually be able to produce power for civilian purposes at reasonable cost—before our non-replenishable fossil fuels are completely exhausted.

So here we have physics up to date. What a tremendous amount of progress in sixty-odd years since 1895! Perhaps we might think that now we understand the "grand underlying principles," and so fall into the same trap as Michelson. But maybe we can learn caution from his example, and keep our minds open. It is indeed "never safe to affirm that the future of physical science has no marvels in store."

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APPARATUS AND FILM

The tools we work with are the means of putting to work the basic physics we have been reviewing.

Roentgen's equipment in 1895 (described in the second chapter), was a far cry from what we now have at our disposal. All of his parts were generally available in physics laboratories, and this allowed confirmation of his work and further experimentation immediately. Nevertheless, manufacturers of tubes and coils were swamped with orders, and there were temporary shortages.

For the first eighteen years after 1895 the tubes used were very similar to Roentgen's original one, though modified and improved in some details. This was the "gas tube" containing a partial vacuum. The degree of vacuum, or "hardness" (Roentgen's own term) was one of the considerable problems of the radiographer—tubes varied widely in hardness, changing with time and with use. Another problem of the early tubes may be inferred from a deadpan remark made by the pioneer Francis Williams: "If the target is not in the right position, it will get there if you shake it a little" (3).

One refinement of later tubes was a "regulator" in an accessory glass bulb on one side of the tube, which contained a substance which could be made to absorb or give off gas—thus controlling the hardness of the tube. A rack of tubes of different hardness was part of the tool kit of every radiologist. Certain tubes were good for head, extremities, trunk, etc. Tubes used continuously lost vacuum pretty fast, some in about three to four minutes, and in therapy it was sometimes necessary to use several tubes for one deep treatment. A good tube cost about $20, and was expected to last for several hundred diagnostic exposures. As to length of exposures, a chest examination in twenty seconds was reported in Philadelphia in 1903, and was considered impossible at first, since most people were using ten minutes.

When W. D. Coolidge brought out his high vacuum, hot filament tube with tungsten target in 1913, it marked a major milestone. Although it has since been refined in many ways (methods of electron focusing, smaller focal spots, rotating anode, pyrex glass, cascading acceleration) it remains the basis of today's tubes. This tube was developed in the Schenectady (New York) laboratories of the General Electric Company, and has added no little to its prestige.

What of the tube's future? No radical changes are at present in sight. Competition from isotopes, of course, exists with cobalt in therapy; diagnostic use of isotopes (principally thulium and cesium) is as yet far from competitive (4).

Since commercial sources of electricity were generally inadequate and unreliable, power plants at the outset consisted of static machines and induction coils. The latter gradually superseded the former due to certain advantages. Rectification was not too satisfactory and various interruptors came and went. The Wehnelt electrolytic interruptor of 1906 represented a step forward, but gave off unpleasant fumes when hot, and subjected the staff to the hazards of handling sulfuric acid. But at best output was erratic and together with temperamental tubes made radiography quite an art. Voltage was generally estimated by length of spark gap and current by its "fatness." Penetration was judged by the time honored method of looking at one's own hand through a fluoroscope screen. It has been said that the good guessers became the good radiologists.

I want to give you a little anecdote about those early days. This story was recounted by Dr. Henry K. Pancoast, of Philadelphia, and took place in 1904 (5).

> "While working in our one room quarters, I was sent for one night to examine a man with a supposed fracture of the spine. He had been struck by a trolley car. In this particular case a negative diagnosis was made by the skiagrapher without the benefit of a roentgen examination. The man was sent to the x-ray room in a bed which would not go through the door into the room. Consequently, the examination, which required about ten minutes exposure for a single plate of the spine in the antero-posterior direction (lateral views were unsatisfactory at that time) had to be made in bed, in the hall. There were no reels in those days, and annunciator wire had to be strung from the coil in the far end of the room to the tube in the hall, supported in between by bandages fastened to the chandelier. The tube was one of the old Queen variety, with the terminals on the outside affixed by sealing wax. It was clamped into a burette holder and placed on a wide board, which had been inserted under the patient's back to keep the plate from breaking. All of this equipment was, of course, on the bed. During the exposure one of the wires began to swing and permitted a spark to ground on the head of the bed. In attempting to quiet this wire with a yardstick, we inadvertently produced a cataclysm. The heated sealing wax permitted one terminal to come off the tube and the wire dropped on the patient. He at once jumped out of bed and the tube went with him. The noise of the breaking roentgen tube and the sensation of sparks hitting him from all directions were more than the patient could bear, and he jumped up, ran upstairs, got into his clothes and left the hospital. After wading through high tension wires and getting numerous shocks on the legs, we finally succeeded in shutting off the machine without further mishap, and of course ventured a negative diagnosis for possible fracture of the spine."

The Snook interruptorless transformer of 1907, with built-in mechanical rectification, was a decided improvement and permitted much better control.

The autotransformer was introduced in 1915.

The Coolidge "radiator" tube of 1917 was self-rectified and tolerated the higher voltages and currents available from improved power sources.

By 1920, mechanically rectified therapy machines operating at 200 Kilovolts and 8 Milliamperes were on the market. However, the idea was not new (6). The "Fleming Valve" of 1901 used the principle of predominant flow from negative to positive with an electron-emitting hot cathode as the source. It was not a practical success until years later when tungsten became available, which could stand much higher temperatures than ordinary metals. Futhermore, a vacuum tube with an electron-emitting hot cathode was patented by Wehnelt in 1904. It was marketed in 1909 and was still selling after 1930.

After 1926, valve tube rectification gradually superseded other methods.

Further refinements have resulted in more and more powerful and gadget-studded machines.

In 1940, D. W. Kerst produced his electron accelerator, or betatron. The first commercial twenty-two million volt unit was sold in 1948.

Incidentally, Coolidge was still alive at the time of this writing, and has published two very interesting reviews of the development of x-ray tubes and generators over the first half of this century in the *American Journal of Roentgenology* (December, 1930; January, 1956) (7, 8).

Several accessory items are worthy of mention.

Intensifying screens were adopted early, and were actually studied simultaneously by physicists in Italy, France, England, and America within the first three months of 1896. By the end of that year Max Levy, in Berlin, was using film coated with emulsion on both sides placed between two screens. Although screens were known and used this early they fell into relative disuse for some years because of problems arising from coarse grain. As they were improved, they came back into favor and general use.

Grids to reduce scatter effect on the film date from 1913 when Dr. Gustave Bucky produced the first stationary one. They have been tremendously improved since, most notably by H. E. Potter who developed the moving grid between 1916 and 1920 (9, 10).

Fluoroscopy, of course, historically antedated radiography—since Roentgen's first recognition of the "new kind of rays" was by means of a fluorescent screen. It is interesting that the name "fluoroscope" (whence "fluoroscopy") was actually coined by Thomas Edison, who leapt on the band wagon so early. Improvements in screens have been made over the years, but the principle has not changed at all since 1895 (11).

Photofluorography was tried in 1897, in the United States. Like so many other techniques, it was pretty crude at first and needed a lot of sandpapering to make it as good as it is today. Photo-timing was devised by Morgan in 1942, specifically for photofluorography.

The idea of recording motion by means of a rapid sequence of exposures likewise occurred early. After all, 1890 to 1900 was the decade when movies were being born. J. MacIntyre, of Glasgow, in early 1896, took serial radiographs of a frog's leg in various positions to show its range. In 1909, Kaestle, Rieder and Rosenthal recorded gastric peristalsis by taking thirteen exposures in twenty-two seconds. During the next three years Lewis Gregory Cole revised the technique to obtain one or more exposures per second (12). Since then innumerable ways have been tested—direct (multiple roentgenograms) and indirect (photofluorography), multiple cassettes, roll film—and improvements are still being made. After ten years of experimental work Ruggles produced a greatly improved roentgen motion picture in 1925, using the direct technique (13).

Image amplification is a phenomenon of only yesterday. It was foreseen in the 1930's, and after considerable work (interrupted by World War II) R. J. Moon published a workable system in 1948. Many improvements have been made, and I think we may still be in the developmental stage. At such a time the radiologist of a small institution rather hesitates to spend the several thousand dollars involved in such apparatus, because it may be obsolete in a few years.

Combining image amplification with cinefluorographic techniques is giving us quite a lot of fascinating new information on function.

Photographic emulsions in 1895 were principally used in the form of the "dry plate" developed by Richard L. Maddox in 1871 (14,15). It consisted of a thin film of silver bromide on glass. Emulsions mounted on cardboard and celluloid were also available but were not much used on account of curling and preference of most workers for the glass plates. Because of the slowness of emulsions generally in use, efforts were made during 1896 and the next few years to improve them. Special plates and emulsions for x-ray purposes were put on the market early in 1896, and many modifications were made over the succeeding years. The greatest improvement stemmed from the use of intensifying screens, which have already been mentioned.

The glass for the plates was obtained from Belgium so that the interruption of supply in World War I provided a considerable impetus for shifting to substitute mounts. Cellulose nitrate was the material used for film base from 1900 on, and although cellulose acetate was known by 1906, it did not come generally into use until much later. By 1920, glass plates were almost out of use, fragility and bulkiness being their chief drawbacks. I have been told that even in 1922, the Johns Hopkins Hospital in Baltimore refused to shift from glass plates and obtained these by sea from New York. This method was apparently more reliable than any other system of delivery at that time.

Cellulose acetate production was greatly spurred during World War I, many uses for it having developed. It was actually available in the form of x-ray film by 1924, but was slightly more expensive than cellulose nitrate, and was thought by some to curl more. The only serious objection to nitrate film was its inflammability. This was recognized and publicly stressed as early as 1923 by many persons and organizations concerned with fire prevention. Special precautions were necessary in storing it, which in an indirect but very real way raised its cost.

This problem came to a head abruptly on May 15, 1929, when a really bad x-ray film fire occurred at the Cleveland Clinic. One hundred and twenty-five people were killed, and some eighty more required hospitalization. The whole radiology department of the clinic was destroyed, to say nothing of damage to the rest of the hospital. This tragic disaster, as well as several lesser fires (in New York, Albany and the University of California) within a short time, startled the public as well as the medical profession into action. People stopped writing articles on how best to store inflammable film and started passing laws prohibiting its sale. The upshot was a general shift to cellulose acetate within a very short time.

I have been told a story about this period to the effect that a prominent x-ray machine maker called upon an important film manufacturer and said bluntly that it was absolutely necessary to put out a safety film. The film man is supposed to have hemmed and hawed, and raised objections. The other gentleman immediately placed an order for $100,000.00 worth of French safety film then on the market. The American company was so shaken by this that it promptly started snapping whips, and within a few months had a good cellulose acetate film for sale.

Improvements since then have continued with ever faster emulsions and better screens. Special varieties of x-ray film have been perfected for different industrial and medical uses.

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LITERATURE AND ORGANIZATIONS

Some idea of the status and progress of almost any field can be gained by noticing the periodicals and organizations which it generates. Certainly these data are important to the historian.

I have mentioned already some of the reactions to the birth of radiology in contemporary newspapers and magazines. Just to amplify a bit, here are some of the scientific journals which printed papers on x-ray in the month of January, 1896:

In *Germany and Austria*— Elektrotechnische Zeitschrift, 1/23/96.
Muenchener Medizinische Wochenschrift, 1/4/96.
Wiener Klinische Wochenschrift, 1/16/96.
In *Italy* ———————— Il Nuovo Cimento, January 1896
In *England* ——————— The Electrician, 1/10/96.
Nature, 1/16/96.
British Medical Journal, 1/11/96.
Lancet, 1/11/96
British Journal of Photography, 1/10/96.
In *America* ——————— The Electrical Engineer, 1/8/96.
New York Medical Record, 1/11/96.
Science, 1/24/96.

Roentgen's first paper on x-rays appeared first in English in the issue of *Nature* for 1/23/96, and then in *Science,* 2/14/96—both, of course, in England.

The *Journal of the American Medical Association* only got on the bandwagon with an editorial on February 15th. In the succeeding months there was a great deal of duplication of effort and conflicting claims of priority in many details. The output of articles was tremendous, and was diffused throughout dozens of medical, photographic, and engineering journals. One American periodical, the *Electrical World* printed 184 articles between January and June, 1896; no one could assimilate all of it. Even though a bibliography of x-ray literature for 1896 and 1897 showed only 5 per cent of the articles to have a medical bearing, hardly one-third of these appeared in medical periodicals. Furthermore, quite a few American physicians published articles in foreign journals during the first few years of radiology's infancy. Thus there developed very early an obvious need for a medical publication devoted entirely to radiology. This need was first met by the foundation of the *Archives of Clinical Skiagraphy* in London, in May, 1896. This journal, through several reorganizations has come down to the present as the *British Journal of Radiology* which we know today. Other journals which were among the earliest, and those of principal interest to us in America are listed here:

American X-ray Journal, St. Louis (May 1897), published until 1906, then superseded by the *American Quarterly of Roentgenology.*

Fortschritte auf dem Gebiete der Roentgenstrahlen, Germany. (Sept. 1897.)

Strahlentherapie, Germany. (1912.)

Transactions (annual) of the *American Roentgen Ray Society* (1900), which changed in 1906 to the

American Quarterly of Roentgenology (1906), which in turn became in 1913 the

American Journal of Roentgenology (1913), which has continued publication to date (name changed, to add *and Radiation Treatment,* 1920; *and Nuclear Medicine,* 1951; significant of the broadening field).

The Journal of Roentgenology (May 1918), quarterly until January, 1920, when name changed to *The Journal of Radiology* appearing monthly. In September, 1923, the name was again changed to *Radiology*, which has continued monthly publication to date.

The Radiological Review (1924), founded as the organ of the Illinois Radiological Society. This journal merged in 1927 with

The Chicago Medical Recorder (1927). In 1936, this became *The Radiologic Review and Mississippi Valley Medical Journal.* This journal still continues publication with a major emphasis on general medicine, but a vestige of its radiologic heritage in an annual radiotherapy number.

Acta Radiologica, Scandinavia. (1933.)

Journal of the Faculty of Radiologists, England. (1953.)

In regard to organizations, the following are the earliest important ones, and those of most concern to us in America. Obviously, there are hundreds more, of all sizes.

Society for Scientific Photography, Berlin. (Jan. 1896.)

The Roentgen Society, London. (1897.)

American Roentgen Ray Society. (1900.)

German Roentgen Society. (1905.)

Western Roentgen Society (United States) (1915), whose name was changed in 1918 to the

Radiological Society of North America. (Nov., 1918.)

American Radium Society. (1916.)

American College of Radiology. (1923.)

Section on Radiology, American Medical Association (1925). Formal establishment of this Section marked the culmination of some five years' intensive effort. This effort may seem strange today, when radiology feels pretty well assured of its importance. In the early twenties, however, radiologists were still on the defensive much of the time, not only in regard to the lay attitude toward them as picture-takers, but against a similar attitude by a large number of physicians, notably the older conservative ones who controlled organized medicine, and whose experience went back to pre-Roentgen days. Also, in those days there were still a lot of non-medical persons providing diagnostic radiographic service of varying quality and in direct competition with radiologists. Recognition by the American Medical Association meant *official* acceptance of radiology as "practice of medicine."

Foundation of the American Board of Radiology (Sept. 1933) by the four national societies (American Roentgen Ray Society, Radiological Society of North America, American Radium Society, and the American College of Radiology) in cooperation with the Radiology Section of the American Medical Association. This Board gave the first examination at Cleveland in June, 1934, and assumed the rôle of certifying well trained radiologists. Within a few years it won general acceptance, although its examinations and certificates have never had legal force.

Society of Nuclear Medicine (United States). (1957.)

In the nineteen-twenties and thirties, voices were raised rather often to urge the union of the various American radiological organizations. Probably the impetus came from the same threats that resulted in pressure upon the American Medical Association to establish the Section on Radiology. Nowadays such a merger does not seem imperative, and the issue is scarcely alive. Certain it is that the four national societies have only gotten together on two occasions. One was the organization of the American Board of Radiology in 1933. The other was the "American Congress of Radiology" at Chicago in 1933, taking advantage of the coincident World's Fair. This congress was intended to initiate a series of Western Hemisphere international congresses, but somehow the way the yankees dominated it did not sit well with the Latin radiologists. Regular Interamerican Congresses began some ten years later. The American Congress itself was apparently a success from the standpoint of most United States radiologists. One valuable by-product was the volume: *The Science of Radiology* (2), which was commissioned in advance with chapters by many noted authorities. It is a mine of historical information about radiology before 1933.

Besides the major national societies there have arisen several regional international groups which meet every few years. The ones of chief interest to us are the International Congresses, initiated in London in 1925, and the Interamerican Congresses begun in Buenos Aires in 1943. These, as well as the major journals, assure that radiologic progress has no boundaries.

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Chapter 5

PROGRESS SINCE 1895—MEDICAL APPLICATIONS

RADIOLOGIC DIAGNOSIS

ROENTGEN DIAGNOSIS constitutes the bulk of medical radiologic practice. It is what I spend most of my time on, and it is the way most radiologists earn their bread and butter. It has taken us quite a while to get here, but I hope you will agree that all of what we have talked about belongs in the picture, for perspective.

I have told you about the enthusiasm that greeted Roentgen's discovery, and something of the rapid exploitation of x-rays in medicine. It is only fair to say, however, that some doctors were very skeptical, and a few even hostile to the new tool. Old fashioned physical diagnosticians, with great confidence in their refined senses of touch and hearing were not always impressed with the primitive radiographs available in those first few years. Sir William Osler was occasionally sarcastic on the subject. A propos of this attitude toward innovations, one may be amused by Dr. Preston Hickey's (1) quotation (in 1924) of a Nineteenth Century comment on Laennec's work: "Although the stethoscope undoubtedly will prove of great value in the examination of cases in military and naval hospitals, still the spectacle of grave physicians listening to a patient's chest is something so ludicrous that it will prevent its universal adoption in private practice."

Even with improving equipment and growing experience in correlating radiography with pathology there remained doubters. As late as 1920, Sir James MacKenzie, the noted cardiologist, allowed himself to be quoted as follows: "Indeed, I am doubtful if an x-ray examination of the heart has ever thrown the slightest light upon any cardiac condition (2)." Cardiologists must be tough birds to convince. In the 1951 edition of his book on heart disease, Dr. Paul D. White accepts x-rays as important in routine cardiac work-up, ranking it fourth in value, below history, physical examination, and electrocardiography.

Who were the people who went into medical radiology in those first few years? It was a mixed bag. We think primarily of the physicians, already mature and in practice, who saw the possibilities and dived in. But there were many non-medical persons, ranging from physicists, engineers, and electricians, through nurses, hospital orderlies, and photographers, to frank charlatans and sideshow exhibitor types. These latter did little to elevate the professional level of radiology, and insofar as they were in the public eye damaged its tone. It took some years before the more professional physicists and engineers either entered medicine fully (like Caldwell and Grubbé), or lost interest and returned to their primary academic or commercial pursuits (like Edison). It took a little vigorous shaking to root out the riff-raff. But by about 1905, the conscientious physicians were in the ascendant and have kept control fairly well since.

At this point I want to draw your attention to three books which contain a wealth of factual and biographical material about the early days and the pioneer radiologists. These are references number 3, 4, and 5 at the end of this section. They are well worth your time, and will give you many details which cannot possibly be included in such a small work as this.

In *American Martyrs to Science through the Roentgen Ray* (5), Dr. Percy Brown discusses the problem of why so many people were injured by x-rays when skin damage, and even epilation, had been recognized in 1896. Furthermore, the *American Electrician* warned the medical community editorially in September, 1897, to take measures to protect the public from misuse of x-rays by those not qualified to handle them.

The answer seems to be largely enthusiasm, and a feeling that x-ray burns were fairly trivial and quickly curable. They were looked on as a nuisance, and only years later did the effect of cumulative damage and late complications sink in. The fluoroscopic "hand test" for gauging penetration of the beam was taught routinely, and was so convenient that for a long time no adequate substitute was sought. There were exceptions—Dr. Francis Williams was particularly cautious, and never developed any skin damage.

The same casual attitude applied to the manufacturers. Only after 1900 was there any effort to build some protection into the tubes. The result, we now know, was a large number of hand injuries, many of which progressed to malignancy. Clarence Dally, one of Edison's assistants, was the first casualty, dying in 1904. A considerable number of others succumbed to similar damages over the next twenty years. A stone shaft in the garden of the St. George Hospital in Hamburg was unveiled in 1936. It commemorates these and many other such martyrs from all over the world. By 1957, the list of inscribed names had reached 302.

Contrasted with the somber notes, here is an example of almost complete absence of any sense of danger from radiation, which was all too common. W. E. Chamberlain (6) tells of an "x-ray party" at the office of his father in 1900. All the family and guests lined up eagerly to take turns seeing their own and each other's bones under the fluoroscopic screen. I shudder now to remember that I did essentially the same thing with an exhibit available to the public in the Deutsches Museum, Munich, in 1931. Other urchins and I used to watch each other's hands and grimacing skulls with great gusto. I do hope they have discontinued that exhibit!

From another and more general stand-point, protection consciousness is necessary all the time, and it is very much the responsibility of every radiologist to see that equipment under his control presents no hazard to the technicians using it and the public—meaning everybody and anybody who may come near it. I remember, just a few years ago, finding a nursery school group of toddlers playing in a sandbox directly outside of a thin wooden partition which was getting the direct beam of a diagnostic machine. This was an army installation, and that machine was doing two to three hundred routine re-enlistment chest films every morning, besides miscellaneous general radiography. We have to be constantly on the alert to avoid this sort of thing. In a way the public alarm about radiation generated in the last few years may be good—if it results in suppressing such things as shoe fluoroscopes and brings sensible regulation of medical radiologic equipment. I may say that this last is long overdue and would actually be a great boon to us as radiologists.

I spoke a few moments ago of correlating radiographs with pathology. This is so important that it requires a special emphasis. It may be hard for us to realize that in 1896, there existed none of the body of knowledge which we almost take for granted today—no reference books, no big teaching radiology departments with experienced teachers and film museums. It took years and years of painstaking correlation of x-rays with clinical medicine, surgery, and pathology to accumulate the basis for radiographic interpretation. And although much knowledge has been gained, one has only to glance at the literature, which every year reports new normal variants and new radiographic signs of disease, to see that the end of progress is not in sight. Pediatric roentgenology (7) has been a particularly fruitful field to acquire understanding of normal and anomalous bony structure. Now the field of geriatrics (8) is developing a new set of norms. This is basic biology—living, rather than cadaver, anatomy.

Roentgen diagnosis at first was confined to natural contrasts—chiefly bones and joints. This field got off the ground fast. As early as February, 1896, intra-oral films of the teeth had been taken by W. Koenig of Frankfurt, and in April of the same year the use of wrist bone development as a measure of bone age was proposed by Angerer of Munich. Bone work has always been a large part of roentgen diagnosis, and what with trauma and orthopedic procedures still is. Some early observers thought radiography never would get beyond this field. But within months the thickest parts of the body became accessible to study, and with improving apparatus definition of structures grew progressively better.

However, the use of artificial contrast is what has really made the scope of radiography mushroom over the years. This, and certain mechanical and electronic improvements we will now explore as we take up the history of several special fields. As we do so we must constantly bear in mind how clinical progress paralleled the technical advances which we have already reviewed.

Before we leave bones, joints, and soft tissues though, mention must be made of artificial contrasts even here. The nasal accessory sinuses were visualized with lead sulfate by 1902 (Weil and Kienböck), and soft tissue fistulae with Bismuth paste by 1908 (Boeck). Foreign bodies, of course, were an important part of radiographic diagnosis from the earliest days—localization being just as important as detection. Means of eye foreign body localization (9) were proposed in April, 1896, by C. H. Williams of Boston, and the classic Sweet method was first described in 1897. Many modifications and improvements followed. A contact lens with standard marks has been used for this purpose since 1926 and has become more popular recently. It seems to be a very accurate way of doing this very delicate task. Bones themselves have pretty well resisted artificial contrast, except for injection of sinuses which extend into them. But joints have been studied with air contrast since 1905, and even occasionally with denser media.

Obstetrics

Prior to 1700, it was generally believed that the pelvic bones separated during labor, thus permitting passage of the fetus.

During the next two hundred years better anatomic studies were made, external and internal clinical measurements were developed as far as they could go, and some understanding of labor mechanism was obtained.

Practical application of x-rays to obstetrics was delayed a little by the thickness of the maternal abdomen, and the limitations of early apparatus. Albert, in Germany, in 1897, was apparently the first to study the female pelvis radiographically from an obstetrical standpoint. He used, incidentally, the same semi-recumbent position to put the pelvic inlet perpendicular to his central ray, which was popularized many years later (1922) by Thoms, and which is often called the "Thoms position" today.

Albert was closely and independently followed by Budin and Varnier, in France. Since these pioneers, innumerable further attempts have been made to improve and refine obstetrical radiographic measurements with special rulers, slide rules, graphs, nomograms, perforated grids, and "teleo" technique. Everyone invented a fancy name for his own special gadget like "pelvi-cephalometer," and many others. All methods, of course, used the same basic geometric principles for correcting the distortion resulting from divergent rays.

Between 1910 and 1920, stereoscopic methods of pelvimetry were explored. These culminated with the work of Caldwell and Moloy in 1932. Unfortunately, their method—while admirable from every technical standpoint—involves special equipment and complicated techniques. Thus it has not been adopted in many small and moderate sized institutions.

The ideal method must be simple, accurate, and inexpensive. The biggest problem with pelvimetry today is the large number of varying methods in general use. Accumulation of uniform clinical experience is thus hampered.

Many methods of measurement of the fetal skull have been tried. There are obvious built-in errors to such mensuration, and the subject remains a controversial one. Quite a few obstetricians are not interested in skull measurements as such; they form a general idea of its size from inspection, and seem to do just as good work as others who measure with micrometers.

Classification and analysis of the normal female pelvis is a subject where radiography has played a significant role. Other diagnostic obstetrical uses of x-ray include: diagnosis of pregnancy, multiple pregnancy, and fetal presentation, position, deformities, and death.

The placenta can usually be located by natural contrast, but artificial ones have been of some use. Cystography is obviously of value. Amniography, angiography, isotope localization, and injection of opaque material into the uterus have all been advocated, but none is generally applicable. Third stage placentography (injection of the clamped umbilical artery) is frankly an experimental matter.

As in every field, methods of roentgen pelvimetry wax and wane in popularity. The trend at the present seems to be to minimize maternal radiation exposure, and make examinations as simple as possible for the problem cases.

Salpingography

The first instillations of an artificial contrast into the uterus and Fallopian tubes appear to be the work of LeLourier in 1912, and of Cary in 1914. They and others used silver protein solutions, colloidal silver, sodium bromide, and thorium, but all of these media proved unsuitable because of damage to the mucous surfaces and resulting complications. Bismuth paste was also used, but it doesn't sound like an ideal medium either.

When Rubin popularized the tubal patency test with air in 1920, attention was drawn away from artificial contrast. Pneumoperitoneum was sometimes used, either alone, or in conjunction with opaque studies.

But when Sicard and Forestier introduced Lipiodol in 1922, it proved such a bland and harmless material that it was at once put to work in a great many ways. (Lipiodol was first developed for the purpose of iodine therapy by injection.) Actual use in salpingography was first reported simultaneously in 1924, by St. Portret (in France) and Heuser (Argentina).

Lipiodol has been the preferred material ever since, and is still commonly used. In recent years, however, aqueous viscous agents (the same type as used for bronchography) have entered the field and seem to be just as good. Probably both aqueous and oily agents will continue to compete for a long time.

Gastro-Intestinal Diagnosis

As soon as the abdomen could be radiographed doctors looked eagerly to see what could be discovered. It was disappointingly little. But it wasn't long before contrast substances were introduced into the hollow viscera.

The first man to experiment with radio-opaque material in the gastro-intestinal tract was W. Becher, of Berlin, who visualized the stomach of a guinea pig with a solution of lead subacetate in March 1896. More animal work was done later in the same year by W. B. Cannon, prompted by his professor of physiology H. P. Bowditch, while still a medical student at Harvard. He used discrete opaque objects—pearl buttons happened to be handy—to watch the swallowing mechanism, and presently tried bismuth subnitrate mixed with food.

Early in 1897, Roux and Balthazard, in France, and Rumpel, in Germany, were conducting studies with bismuth on humans. The technique spread from then on, though hampered in the early years by limitations of equipment. Much of the early exploration was done in Boston by Francis H. Williams, working with Cannon. Dr. Williams' book *The Roentgen Rays in Medicine and Surgery,* which appeared in 1901, reported his gastro-intestinal investigations.

Fluoroscopy was used predominantly in these early studies (15, 16), since radiographs of the abdomen took ten to thirty minutes. During the ten years following 1895, enough improvements in x-ray machines were achieved to make radiography of the gastro-intestinal tract more practical. The first serious skin damages of the early fluoroscopists were also becoming apparent. The result was a temporary decline in the popularity of fluoroscopy, though it later found its proper place.

Around 1904, there arose a fairly sharp cleavage between those who emphasized radiographs, and those who stressed fluoroscopy in study of the stomach and intestines. One of the former was Lewis Gregory Cole, of New York, whose gastro-intestinal series ran to sixty, or so, films. Even today one finds some divergence of attitude toward films and fluoroscopy from one radiologist to another. Of course, the performance of a gastro-intestinal series is really an art, and each of us has to perfect his own technique in his own way.

Much of the work up to 1910, consisted of acquiring a knowledge of the normal. From then on more and more specific pathology was recognized.

In 1904, retrograde filling of the colon was introduced by Rieder. Previously it had only been observed by means of the opaque meal from above.

In 1906, double contrast examination of the stomach was described by Holzknecht and Braun, using Seidlitz powders. Air contrast enemas were not tried until 1923 (Fischer).

In 1910, barium began to displace bismuth, since it was just as good, and cheaper. In the same year, L. G. Cole of New York coined the term "duodenal cap," which we use so freely today, describing it as sitting "like a stocking cap on the pyloric end of the stomach." He also recognized persistent duodenal deformity as significant of ulcer.

A friend of Dr. Cole has been kind enough to lend me some extremely interesting material about his career. The following (17) is a rather dramatic story concerning this extraordinary man.

In 1913, Dr. Cole tried to convince his friend Dr. George E. Brewer of the value of radiographic examination of the gastro-intestinal tract. Dr. Brewer was a very distinguished surgeon who had made his mind up that gastro-intestinal roentgenology was no good. Besides, the Mayos said so, too.

"That," said Cole, "is because you don't do it right," and he went on to offer a test. Dr. Brewer was to work up a series of surgical gastro-intestinal cases fully in the usual manner, but also send them for radiographic study—with no clinical data, and at Cole's expense. Dr. Cole would do the study, even avoiding quizzing or palpating the patient himself. The radiographic report was to be sealed, and read aloud to the assembled students and other operating room spectators just before surgery. Dr. Brewer would operate regardless of the report.

Dr. Brewer accepted the challenge, even though feeling that he was taking advantage of Cole, and allowing him to incur needless heavy expense. But Cole had said, "I can afford it better than you can afford to operate on these cases without knowing beforehand what you are operating for."

The word got around, and the amphitheater was crowded when the first such case came to the table. The clinical work-up adequately indicated exploration, but there was no statement of what lesion was expected. The x-ray report made a positive diagnosis of duodenal ulcer. Sure enough, the patient had a definite duodenal ulcer.

The next case was also correctly diagnosed, and on the third, Cole described a gastric carcinoma, which he called inoperable. Dr. Brewer was annoyed, operated anyway, and found an inoperable tumor.

But on the next case, with plenty of symptomatology, the roentgen findings were quite negative. The report was read, the audience was in an uproar. Brewer took a deep breath and opened the abdomen. The stomach and duodenum were normal. He did, however, find a diseased gall bladder containing stones, and removed it.

And this went on for a total of twenty-seven cases. By that time Cole's batting average was twenty-four correct diagnoses, and Brewer threw in the sponge. He was convinced, and lent his prestige to advertising the importance of gastro-intestinal roentgenology at a meeting of the American Surgical Association in 1914. Although the assembled surgeons were not too impressed at that time, the tide was turning. In the journal, *Surgery, Gynecology and Obstetrics* of May, 1914, the following statement appeared: "The time seems near at hand when chronic surgical lesions of the stomach should not be operated upon without previous roentgenological examination, if it is possible to obtain one."

Haudek (in Germany), in 1910, first described the roentgen pattern of gastric ulcer, and in 1911, Pfahler (United States) and Holzknecht (Germany) independently described gastric carcinoma.

Around 1913, lead-rubber aprons and gloves began to come into use for protection during fluoroscopy. They were recommended by Dr. Russell Carman, who discussed the problem of differential diagnosis of gastric ulcer and carcinoma, a puzzle which we have by no means solved yet. Dr. Carman published *The Roentgen Diagnosis of Diseases of the Alimentary Canal* in 1917, and described the "meniscus sign" in 1921.

There has continued a steady but unspectacular output of work, expanding further and further our understanding of gastrointestinal pathology. There is far too much to review here. The only landmarks of the last thirty years that I shall mention are: the first description of regional enteritis by Crohn, Ginsburg, and Oppenheimer in 1932; studies of the pancreas through its relations to the barium-containing stomach and duodenum (Frostberg's "epsilon sign") in 1938, and the work of Dr. M. H. Poppel (18) since then; and the modern understanding of megacolon, based on anatomic studies done in 1940 by Tiffin, and elucidated radiographically by Swenson and others from 1948 on.

A technical development worthy of note is the Miller-Abbott tube, which appeared in 1936. Aside from its use in treatment, it has contributed a great deal to study of the small bowel under abnormal conditions.

Biliary Tract Diagnosis

Biliary disease being as common as it is one cannot be surprised that the first roentgenologists hoped to visualize calculi through natural density. It took some time to discover through experience that only about forty per cent of gallstones are opaque enough to see without artificial contrast.

In 1897, when the human torso was a roentgenographic terra incognita, William Osler asked skeptically, "can you see gall stones with the x-ray?" And no one could give him an answer. But in a matter of months Buxbaum, in 1898, did demonstrate them.

As apparatus improved, the gall bladder itself was occasionally visualized, and at first this was thought *per se* to be abnormal. It was only around 1920 that it became clear that many gall bladders could be distinguished in good abdominal films.

Lacking artificial contrasts the early radiologists sought secondary signs of gall bladder disorder—evidence of pressure on the duodenum, stomach or colon which could be opacified, and irritability of these structures. Inevitably these tenuous signs were exaggerated. We are still in that stage in regard to the pancreas.

Biliary diagnosis really came of age in 1924, when Graham and Cole, of St. Louis, first succeeded in making the liver excrete a harmless opaque substance, and thus fill the gall bladder with bile artificially dense enough to give good contrast. The roots of this major landmark go back to 1896, when Sehrwald, in Germany, showed that halogens were radio-opaque. Then, in 1905, Abel and Rowntree had shown that phenoltetrachlorphthalein injected subcutaneously was excreted solely in bile, and developed thence a liver function test. Graham and Cole used calcium and sodium salts of tetrabromphenolphthalein in divided doses intravenously, and took serial exposures over a two day period. By 1925, they were giving the dye orally, and many variations of technique developed over the succeeding years. Iodine was substituted for bromine, because of lower toxicity, and a number of chemical refinements have been made. There is still room for improvement, since even Telepaque (iopanoic acid, 1949) has considerable laxative side effect.

The result of making cholecystography practical was to expand vastly our preoperative understanding of gall bladder function and disease. Operative and post-operative cholangiography were natural further developments. Intravenous cholecystography and cholangiography, which were introduced with "cholografin" (21) in 1953, represent a striking technical improvement, though visualization of the ducts is often possible with the oral technique.

Urology

Urology hardly existed as a specialty before the invention of the cystoscope and the discovery of x-rays. Imagine being limited to abdominal palpation, rectal examination, and urinalysis!

In 1887, Max Nitze developed the first cystoscope, which of course has been improved repeatedly since. With this the bladder became accessible and ureteral intubation was soon added. Some ureteral probes were wax tipped, to register scratches if a calculus was encountered.

During 1896, surgeons interested in urology tried to visualize urinary stones in films of the abdomen, and a Dr. MacIntyre of Glasgow deserves credit for demonstrating a ureteral calculus radiographically for the first time. Only large and dense stones could be identified, with the feeble equipment and twenty minute exposures required.

By 1897, Tuffier radiographed opaque ureteral probes in situ—the first urologic study with artificial contrast.

Ureteral catheters had already been used for collection of differential urine specimens. It was not difficult to make these satisfactorily opaque, and in 1905, Voelcker and von Lichtenberg (Berlin) performed the first retrograde pyelogram and cystogram, using a silver protein solution. This medium caused mucosal irritation and was abandoned in favor of thorium, and later the iodine dyes.

Cystograms with air were made by Wittek in 1902, and this method still has some usefulness. Pyelograms with oxygen were tried in 1907, by Burkhardt and Polano, but were so inferior to the denser media that the procedure fell out of use. However, air contrast continued to be tried from every conceivable direction—in the stomach, in the colon, and in the retro-peritoneal space. In 1921, Rosenstein and Carelli independently performed retroperitoneal peri-renal air injections. This technique still has some place, though superseded to a considerable extent by the presacral gas injection described by Ruiz Rivas in 1948.

Excretory urography was the next major step. In 1923, Rowntree, Osborne, Sutherland and Scholl, at the Mayo Clinic, did some of the earliest experimental work on animals, using 15 per cent sodium iodide.

Practical intravenous pyelograms in humans were first made by Roseno, and by Binz, in Germany. The latter developed "uroselectan" (iopax) in the course of investigating anti-luetic drugs, and the application to urologic diagnosis was almost accidental.

Once the break-through was made progress continued, with a succession of improved dyes—neo-iopax (1929), diodrast (1932), uroken (1953), hypaque (1954), and others.

During the past ten years, two techniques which we will presently take up separately have been applied to the study of the kidneys. These are angiography (intravenous or translumbar aortography) and tomography; sometimes they are even used simultaneously, and with presacral air as well.

Another urologic opacification method, of relatively minor importance, is injection of the vasa deferentia and seminal vesicles (23). Although retrograde injection is sometimes feasible, vasotomy is generally required. This was practiced first by Belfield (24) in 1913. He used collargol, a silver protein solution, which has been superseded by lipiodol and its successors. One side effect of the test was that for several days after a collargol instillation the patient had black semen. This gave rise to no little alarm. One man thought it might produce Ethiopian offspring.

Central Nervous System

Little of a precise nature could be done to analyze central nervous system structures radiographically until artificial contrast was introduced. Bony changes of the skull and spine, and pineal displacements tell us something, but not very much.

Luckett, in 1913, was the first to describe air in the cerebral ventricles following skull fracture. Several more such cases were reported in the next few years.

But it was Dandy who, in 1918, deliberately injected air through fontanelles or burr holes, and coined the term "ventriculography." One year later he described air injection by the lumbar route. The technique was at once adopted by others, and the cisternal approach was also employed. Dandy himself preferred ventriculography, and it was only several years later that lumbar "encephalography" became popular.

Air has remained the standard contrast material for the brain, though lipiodol and other opaque substances have been used a little in Europe.

The first myelogram was done by Wideroe in 1921, using air.

In 1922, Sicard and Forestier successfully used lipiodol for myelograms. And although there was a little dissatisfaction with it —failure to absorb and questionable secondary arachnoiditis— lipiodol remained the standard medium for many years. Indeed, it is still preferred by some people today. Other oils were tried, and around 1933, thorotrast was used in a few cases.

In 1944, pantopaque was developed, a medium less irritating than any previously used, and somewhat more absorbable. It has become the preferred material in the United States. In Europe abrodil, produced in 1931, is generally used, and seems to do just as well.

In 1948, Lindblom (27) described injection of the nucleus pulposus of the intervertebral disk—"diskography." In ten years, this technique seems to have held its own with a few enthusiasts, but has not been generally adopted, particularly in the United States.

Angiography of the cerebral blood vessels was described by Egas Moniz of Lisbon, in 1927. He used sodium iodide, and later thorotrast. The method has been refined through integration with facilities for rapid serial exposures, and improved contrast substances.

These contrast studies are by now firmly built into neuro-surgical diagnosis. If we took away from him air and angiographic studies of the brain, and pantopaque examination of the spinal canal, the neuro-surgeon would be almost as badly off as a urologist deprived of excretory and retrograde pyelography.

The Chest

Roentgen examination of the contents of the chest was also taken up early. Even though it was much more easily penetrated than the abdomen, fluoroscopy was used almost exclusively for several years, and only diminished in popularity as radiographs became fast enough to be feasible without respiratory motion (1905).

The first ten years, too, saw extensive work of the same sort I have already mentioned—correlation of roentgen shadows with anatomy and pathology.

In a paper titled "Notes on X-rays in Medicine," presented by Francis Williams in Boston on April 30, 1896, he described his preliminary experiences with chest fluoroscopy. A great deal more material was presented in his book, *The Roentgen Rays in Medicine and Surgery*, in 1901.

Stereoscopy, an optical phenomenon already well known, was introduced to radiography by Elihu Thompson in 1896, but of course could not be applied in a really accurate manner until equipment was improved.

Orthodiagraphy was described by Moritz in 1902, and flourished chiefly during the predominance of fluoroscopy.

Teleo-roentgenography was proposed by Koehler in 1905, but here again extensive application was not feasible till some years later.

In 1911, Kymography was first used in Germany.

By the time the Coolidge tube became available, in 1913, chest roentgenology was definitely coming into maturity. Despite the hesitancy of old line doctors (as noted) it was more and more entering into significance in diagnosis and management of pulmonary and cardiovascular disease.

In 1916, the Trudeau group of chest physicians first established a classification of tuberculosis according to roentgenographic extent. With only slight modifications we still use this same classification. Obviously this acceptance of radiographic criteria was based on accumulation of experience over quite a few years. In contrast to the chest physicians, the United States army was not ready to recognize the value of chest radiography. I want to tell you something about this (17).

When we got into World War I in 1917, Dr. Lewis Gregory Cole already had more than ten years' experience with roentgen studies of pulmonary tuberculosis. He and others tried to interest the army in routine chest films of recruits. The army was not impressed, and objected that facilities and trained roentgenologists were inadequate anyway. They based the diagnosis of tuberculosis on post-tussic râles. They cited the "enormous cost." But it has been established that every tuberculous recruit in World War I cost the government $15,000 over the subsequent twenty-five years.

Cole even ran a pilot study (at his own expense) examining a group of draftees and reported some twenty-five positive for tuberculosis. Of these several were inducted anyway. In 1935, a friend of Dr. Cole looked up what had happened to these twenty-five men through the Veteran's Administration. Of those that could be traced one died in combat, and the others came down with obvious tuberculosis sooner or later.

This study appeared in the *Journal of the American Medical Association,* October 19, 1940, (*115*:1371), and played an important part in convincing our military authorities of the wisdom of taking chest films on all men taken into the armed forces during the second World War. Up to that time they had *not* included such a procedure in plans for any future mobilization.

In the early 1920's cardiac mensuration was developed to a high degree. So high, in fact, as to exceed practicality, and such measurements are only used nowadays in a few problem cases.

Study of the heart by its natural contrasts, in all positions, and with measurements had gone to its limit by the mid 1920's. Further development required the artificial contrast of angiography, which has opened an entirely new chapter in cardiac diagnosis. But we are going to look at angiography separately.

Concern about occupational pneumoconiosis became strong in the 1920's (29). Silicosis was made compensable in the United States after 1930, and considerable study was applied to this field. It is still expanding, as we can see by the literature of recent years.

Official recognition of this class of occupational hazards came rather late. A relationship between certain types of work and pulmonary disability had been noticed for many centuries. Even Hippocrates described miners who "breathed with difficulty" and were "of pale and wan complexion." This is another instance of an empirical observation waiting for modern technology to give it substance. And in this case roentgen studies of the chest played a considerable rôle, even though the characteristic changes had been described by pathologists many years before.

Lung carcinoma, a field where we still do not manage very well to make early diagnosis, was emphasized more than thirty years ago when Hyder and Holmes (1927) viewed with alarm its rising incidence.

Artificial contrast applied to the lungs goes back to 1904 when Hickey did cadaver studies with opaque materials in bronchi and pulmonary vessels. Application to the living patient, however, came after World War I, with the work of Chevalier Jackson, who did so much to develop bronchoscopy. Suitable anesthesia was very important too. Jackson occasionally marked bronchial lesions with metal clips, through the scope, to aid later roentgen visualization and localization. In 1918 he insufflated bismuth powder through the bronchoscope.

In 1920, an oily suspension of bismuth subcarbonate was tried, but when lipiodol appeared in 1922 (introduced by Sicard and Forestier) it took over, and was the standard medium in bronchography for many years.

In 1938, viscous aqueous media were proposed, since they were more readily absorbed. These have gradually been improved and have acquired a considerable popularity, perhaps more in Europe than in this country. Lipiodol, however, continues to be widely used.

Body-Section Roentgenography

There is no uncertainty about the physical basis of body-section roentgenography. We all know that everything is blurred out except the selected plane which alone does not move relative to the film and the x-ray source.

But almost everything else about it is very confused. The trouble is that the idea occurred separately to several people at about the same time. They, their collaborators, and followers rapidly developed slightly varying practical applications and each one was given a distinct name. At least eight of these names have been in general use at one time or another: Tomogram, laminagram, planogram, stratigram, body-section, Roentgen section, biatome, analytical radiograph. The first three are in general use in the United States, and which term one prefers depends on where one trained.

These different investigators (about ten, in all) were each fired with zeal and pride of authorship. They put on an unseemly, and even acrimonious, wrangle over priority during the early 1930's. Any account will be subject to objection from some quarter, but the following is the best chronology I have been able to sift out.

In 1914, Mayer experimented with a moving tube and stationary film in trying to separate the heart shadow from the rest of the chest.

Between 1917 and 1921, Bocage worked out the theoretical basis of tomography, and in 1921, took out a patent on it in France. Unfortunately he let the project go for some time, and only reported a working apparatus in 1938; by that time several others were on the scene.

Later in 1921, also, two other Frenchmen, Portes and Chausse, took out a French patent, but did no practical work.

In 1931, des Plantes published the first paper on practical clinical application of tomography. He also claimed to have been thinking about it and working on it as far back as 1920. Des Plantes emphasized a spiral type of tube motion, and thought it superior to the linear motion of Bocage.

Several other men (Bartelink, Kieffer, Vallebona, Andrews, Grossman, and Chaoul) deserve mention for independent work and suggestion of improvements or other types of motion.

The most recent variation, appearing just a few years ago, is the multiplane cassette, whose usefulness is still being explored.

Tomography has been most extensively and productively used in study of the lungs, of course. But it has been successfully adapted to many other sites. Detailed analysis of the kidneys, in conjunction with angiographic and/or air studies, has already been mentioned. It has also been useful in special examinations of the gall bladder, many bony conditions, and by giving greatly improved visualization of the midline sagittal plane contributes significantly to air studies of the brain.

Angiography

The injection of opaque substances into vascular channels, which is one of the most rapidly expanding areas of radiology in the past few years, also has roots in roentgenologic antiquity. In February, 1896, only a few weeks after Roentgen's first announcement, Dutto filled arteries of a dead animal with plaster and radiographed them. This experiment was soon followed (independently) by Haschek and Lindenthal, who reported injecting metal salts into the arteries of an amputated human hand and forearm. Again, application to the living patient had to wait for improvements of apparatus and suitable contrast substances.

The first angiogram of a living patient was made in 1923, by Beberich and Hirsch, using 20 per cent strontium bromide in a peripheral artery. This was closely followed by a femoral arteriogram with sodium iodide by Brooks in 1924.

Next came cerebral arteriography in 1927 by Egas Moniz, already noted.

In 1929, dos Santos described lumbar aortography.

In the same year, Forsmann catheterized his own right atrium through an arm vein. Injections through such a catheter were tried and good radiographic studies were obtained in 1931.

Really satisfactory angiocardiograms, visualizing both sides of the heart were developed by Robb and Steinberg in 1938.

Naturally, with rapidly moving boluses of dye through vascular channels such technical advances as rapid cassette changers, cinematography, and image amplification became necessary to get full value out of the examination. Without them angiography would drop back to a level that would seem primitive indeed by today's standard.

Although cardiac and brain surgery have been the chief beneficiaries of the diagnostic advances made possible by angiography, the method has many other uses.

Peripheral arteriography and venography have been practiced for over thirty years.

Visualization of the splenic and portal circulations, which has been studied so much just recently (33, 34), actually goes back in history to intravenous injection of thorotrast in 1929 by Oka, and direct splenic puncture in 1930 by Radt. Thorium studies of the liver were of very limited value, but they did enjoy a brief vogue.

Visualization of the placenta (1952) has been mentioned briefly.

Localization of parathyroid adenomas has been reported (1954) by angiographic technique.

Contrast injection of the uterine arteries was reported in 1955.

Lymphangiography was first reported in 1931, using thorium dioxide (35). Little came of this right away, but the subject has been revived recently. Several articles have appeared describing visualization by means of iodine dyes (36).

The last word in angiography has certainly not been written. We can confidently look forward to new applications and refinements of old ones. A most exciting possibility, recently suggested (37) is "particulate" contrast material—which will be seen as discrete dots, rather than a more or less diluted dye bolus. This idea is not as far-fetched as it might at first sound, and if feasible would open up entirely new areas in the study of hemo-dynamics: e.g., turbulence, flow rates, velocity, and cardiac output.

Radiography without an x-ray machine has already been mentioned briefly, but must be included in any complete review of diagnosis. These radioactive sources are of limited interest to medicine at the present time, though they are of some importance in industry.

The use of radio-isotopes as tracers and dilution measures also belongs in the over all picture of diagnostic radiology. These will be discussed in a separate chapter, since historically the various branches of radiology may best be considered individually.

This sketchy review has merely touched the peaks of the development of clinical diagnostic radiology. I hope it indicates the way this tool has entrenched itself solidly across the spectrum of the medical specialties. X-ray is indispensable to every branch, even to the psychiatrist who uses it to rule out organic neurologic or other disease (if only as a form of reassurance to his patient), and checks the spine for fractures before and after shock therapy. Veterinary medicine has made ample use of radiography, too, though restricted to natural contrasts most of the time.

However, while we preen ourselves over the wide usefulness and indispensability of radiology (38), it is well to reflect that knowing our limitations is equally important. These are not only technical and physiological, but personal as well. It is sobering to read studies made during the last few years (39, 40) which demonstrate that the same man may interpret a borderline chest or gastro-intestinal study quite differently on different days, and that even well-trained and experienced roentgenologists may give varying opinions on independent readings. Of course, exactly the same problem exists with electrocardiograms (41).

When I was first assembling this material for a lecture, I thought I would mention, as an example of a *recent* development, "telognosis," or the transmission of roentgenograms by wire over a long distance. There was some little publicity about this when Gershon-Cohen proposed it in 1949 (42). Alas, like many other things, it was merely revived. The same thing was done, though apparently with less fancy apparatus, in 1925 (43). At neither time did the technique catch on. Although it may have some usefulness, I doubt if it will be great. I think the roentgenologist must do more than study radiographs and report what they show; he should, as a doctor, confer with the referring physician and discuss the roentgen findings in the light of the clinical picture. This may require seeing the patient and his chart, and frequently necessitates more of a conference than can be conducted over the telephone.

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RADIATION THERAPY

Roentgen therapy is generally considered to have come of age around 1920, when the Coolidge tube had been improved to a point where it could stand 200 kilovolts. But first we will glance at its childhood, when 50 to 75 Kv was the rule.

In 1895 Emil H. Grubbé, in Chicago, was engaged in making laboratory glassware, including Crookes tubes. He was also studying medicine. He got the news of Roentgen's discovery as soon as anyone in the United States, and being familiar with cathode rays started experimenting right away. He gave himself radiation dermatitis in January 1896, and in collaboration with a physician friend (R. Ludlam) lost no time in trying x-rays on a case of advanced ulcerated breast carcinoma. One of his professors (T. E. Gilman) suggested it as a form of cautery, to burn away diseased tissue. His interest was strong enough to make him continue with the work long after receiving his medical degree. Eventually he contracted malignant skin damage of the face and hands, and gradually lost his left hand, piecemeal. After some ninety-three surgical operations he finally died (of pneumonia) in March, 1960, at the age of eighty-five. He was by no means the only man to demonstrate that local malignancy due to radiation has no clear correlation with whatever life-shortening effect there may be.

Others noted skin effects within the next few months, often as a result of the long diagnostic exposures necessary. Thus Daniel, at Vanderbilt University, described epilation in April, 1896, and some people hailed this as possibly putting an end to shaving. Another observation from diagnostic work, in November, 1896, was radiation sickness, due to repeated exposures for kidney stones totalling some two hours.

In February, 1896, Voigt, in Hamburg, treated a nasopharyngeal carcinoma; in July, Despeignes, in Lyon, treated a carcinoma of the stomach; and in November, Freund, in Vienna, treated a hairy mole.

There was a tendency at first to confuse x-ray effects with those already familiar in ultra-violet light therapy. Because a brisk erythema was considered necessary for beneficial effects from the latter, the same idea was carried over to roentgen therapy.

The damaging effects of x-ray—in sufficient dose—led during 1896 to attempts to kill bacteria. Results were equivocal, and since there seemed to be some plausibility to the idea, some doctors went ahead and treated inflammatory conditions anyway. Treatment of skin diseases and tuberculosis started early and has continued to be popular for a long time. After about 1906, therapy of tuberculosis became pretty well restricted to lymph nodes, sinuses, bone, and skin. Tuberculous cervical nodes are still occasionally treated with x-ray today.

People have tried x-ray for just about everything. Because early workers noted irritation of the eyes after prolonged staring at unprotected fluorescent screens a trial of therapy for blindness was made, and there was a brief flurry of excitement about this. Epilepsy and syringomyelia are other conditions for which radiation treatment was tried, and aroused transient enthusiasm.

Carcinoma of the cervix uteri was first treated in 1902. Efforts were made then and in the next few years to apply treatment through a speculum, and by means of a special tube inserted into the vagina.

Sterilization of gonads by radiation was first noted by Albers-Schoenberg in 1903.

Hyperthyroidism was first treated in 1904, and this type of roentgen therapy is still occasionally used successfully when other measures are unavailable or contra-indicated.

Treatment of the thymus was also introduced in 1904, and continued to be popular until the latter 1920's. Now the patients so treated are haunting us with the possibility—still controversial—that the treatment may have influenced the later development of thyroid carcinoma. People are also turning up every year with permanent skin damage and/or cancer resulting from unskilled therapy of acne, warts, and removal of superfluous hair in beauty shops.

During the first decade of this century almost every form of malignancy was subjected to radiation therapy, as well as numerous benign diseases (e.g., herpes zoster, rheumatism of all forms). There was some temporary discouragement with radiotherapy, due to many disappointing results and the increasing number of injuries coming to public attention. Some voices were even raised to demand its abandonment. But progress of apparatus and clinical experience, as well as a moderate number of good results kept a few men interested and active—despite the difficulties already mentioned resulting from temperamental tubes and power plants.

At the very outset therapeutic technique consisted of setting the tube in such a way as to irradiate the lesion most directly. By 1903, cross-firing fields were employed. This means of obtaining maximum tumor dose while distributing skin dose has been further developed over the years with multiple fields and rotation (first suggested by Pohl in 1906) of the patient or radiation source. Another skin-sparing method, the grid technique (7), was first used in 1909. It did not attract much attention then, but was revived in the 1930's and has had some popularity during the last ten years.

Perhaps the major landmark of this first decade was the announcement by Bergonié and Tribandeau, in 1904, of their findings regarding tissue response to radiation. They stated that the effect of radiation on living cells is more intense (a) with greater reproductive activity, (b) longer mitotic phase, and (c) less morphological and functional differentiation. This summary has since been found to be an over-simplification, but it has assumed the stature of a "law," and does—in a general way—underlie the whole theory of radiation treatment of neoplasms.

Radiology has moved on to uncover a wealth of material about cell sensitivity and factors related to the effects of x-rays. Important work was done by Lacassagne in 1910, by Régaud in 1914, by Shields Warren in the 1920's, and by many others.

The whole large field of time-dose-field size relationships (8, 9) is a very complicated matter. A lot of work has been done on it, with vegetable cells, ascaris eggs, chick embryos, human skin erythema, and analysis of clinical data.

In 1927, Régaud and Ferroux noted that they could sterilize a rabbit testis with several exposures, but not with one unless they severely damaged the skin.

In 1932, Coutard emphasized the idea of protraction, and largely as a result of his work the usual deep therapy course came to be given daily over a period of several weeks. Coutard believed that low intensity was important (about 2 r/min.), but subsequent work showed no difference in effect at dose rates varying as widely as 12 to 1273 r/min.

Strandquist, in 1941, showed that effective doses which could be tolerated fell in a rather narrow band ascending toward the right on a graph where dose (ordinate) was plotted against time (abscissa) on log-log paper. This work, while it does not take into account field size, and cannot be extrapolated indefinitely in either direction, was nevertheless extremely valuable, and over the usual range of dosage for tumors shapes much of our treatment policy today. But there is still a lot of work going on and we are far indeed from having all the answers.

All of these studies, plus those on ways of reinforcing radiation damage to tumor, or improving patient resistance, or modifying the tumor bed by drugs or any other means, are directed toward increasing the differential effect of radiation on tumor versus normal tissue.

During the second decade of this century therapy moved ahead slowly, being helped most by the development of the Coolidge tube and improved power sources. By 1920, voltages up to 200 Kv were usual.

A monograph by Friedrich and Kroenig emanating from the Frauenklinik at Freiburg, in 1918, is important to note. It was the first really systematic correlation of the physics and biology of radiation therapy, and set a new standard for evaluating treatment results.

From 1920 on, what we think of as "conventional" deep therapy became established around 200 to 250 Kv. Superficial therapy, for skin and some eye conditions, remained in the 75 to 120 Kv range.

A very wide variety of non-malignant conditions have been given radiation treatment, mostly on an empirical basis. It is a fascinating field, perhaps because so miscellaneous. Mode of action in this group includes suppression of endocrine, glandular, lymphoid, or fibroblastic activity, lysis of inflammation through mechanisms not well understood, and pain relief by means frankly obscure. This unscientific taint has kept benign therapy rather at a disadvantage, in some way not so respectable as cancer therapy. But these diseases form a very respectable volume of therapy in most general hospital x-ray departments, and the relief of suffering is pretty important to the patients. Many diseases (chiefly the infections) have dropped away from this roster due to development of more specific methods, but a large number remains, and it is not unreasonable to expect new applications of benign therapy to be found.

The major tendency since 1920, has been a constant striving to increase the destructive effect of radiation on tumors. We have already noted some approaches to this problem through trying to devise optimum time-dose relationships, multiple fields, rotation, scanning, and grid techniques. Drug or chemical potentiation of radiation has been looked into a little, and we are entitled to hope for considerable progress here. Measures to control radiation sickness and minimize such adverse effects as pulmonary fibrosis are fairly good at present, and these permit us to carry dosage farther than might otherwise be possible. Incidentally, fibrosis of the lung as a complication of therapy was recognized as early as 1922 (10), when deep therapy was very young.

But the other major area of advance has been the development of progressively more penetrating beams, from ever higher voltages, and finally the betatron (1940) and synchrotron (1945) which take us up to 70 megavolts. This tendency has helped substantially to improve the effective tumor dose. Still, we must never be intoxicated by big numbers. No improvement in biological effect from a higher energy beam has been demonstrated. And along with the advantages, which you know, have come new problems of exit dose and integral dose to cope with. The 250 Kv therapy machine is still today the standard work horse of the average medium-sized hospital.

An understanding of radiation quality was built up slowly. Roentgen had described (in his third paper) adding filters to "harden" the beam. Spark gap measurements were generally used at first, then various filters came into use, and in 1913 Christen proposed the "half-value-layer."

Dose determination was very confused in the early years (11).

Several inaccurate formulas based on physical factors were set up, like this one:

$$\text{Dose} = \frac{\text{milliamperes x (spark gap)}^2 \text{ x time}}{\text{(target-skin distance)}^2}$$

Chemical and photographic methods were tried, but the epilation or erythema dose came to be standard.

Between 1896 and 1900, Roentgen himself noted some chemical effects of x-rays.

In 1902, Holzknecht described his "chromo-radiometer," measuring in "H" units degrees of discoloration of a mixture of potassium chloride and sodium carbonate. Three H's equalled one erythema dose.

In 1904, Sabouraud and Noire utilized the colors of barium platinocyanide, and came up with an "S-N" unit equal to 5 H.

Other methods measured precipitation from solution, degree of photographic development, brightness of a fluorescent screen, or change in resistance of an electrical cell. Up to 1930 there was a bewildering array of H, Ha, x, K, F, E, e, and several R units used, and the only common denominator to which they could be equated was the erythema.

A grim picture was painted by James Ewing (12) when reminiscing (in 1933) over the decade before 1920: "At one period the prescription of (therapy) dosage was so uncertain and the results apparently so capricious that all one could really do was to place the patient under the machine and hope for the best. Patients were burned from unexpected leaks, and on one or more occasions, it is said, they were actually electrocuted on the treatment table."

While all this was going on a few people were interested in the ionization effect of x-rays. After all, Roentgen had noticed it originally. In April, 1896, W. L. Robb discussed measurement of x-ray induced ionization by discharging a condensor. Villard, in 1908, proposed a unit of radiation measurement based on ionization almost identical to that which was eventually adopted as the "roentgen."

In 1910, Duddell proposed a measurement of energy absorbed, thus anticipating the "rad."

Further work on ionization was done by Szilard around 1914. Duane, Friedrich, Christen, and others kept the idea alive. Theory preceded instrumentation, as so often happens, and it was only during the 1920's that satisfactory ionization chambers became available.

The second International Congress of Radiology at Stockholm in 1928, received a report defining the "roentgen," or "r" unit of ionization and recommending its adoption. Gradually the advantages of this method were appreciated but it took nine more years to become official. The Fifth International Congress (Chicago, 1937) finally accepted the "r," recognizing the weight of the experimental work with measuring instruments, phantoms, and isodose patterns during the preceding twenty years.

In 1940, Mayneord introduced the "gram-roentgen" as a measure of integral dose.

In 1953, the "rad" was accepted by the International Committee on Radiologic Units, and defined as 100 ergs per gram.

Since then we have "reps" and "rems," and can probably look forward to more specialized adaptations of the versatile "roentgen."

RADIUM

It has been a little difficult for me to decide just how to present the rôle of radium in radiology. To set it apart from radiation therapy must seem artificial. Still, radium has had a distinct career of its own, and I do think it merits separate consideration.

We have already been in at the "birth" of radium in 1898. We have traced Marie Curie's further work with it through the measurement of its atomic weight in 1907, its purification as metal in 1910, and the preparation of the international standard specimen in 1911. We have also mentioned the experiments of Walkhoff and Giesel in 1900, as well as Becquerel's and Pierre Curie's skin burns. To be sure, in retrospect, the lung cancers of the Bohemian miners who worked the central European pitchblende deposits must be regarded as a result of their exposure to radioactivity.

Application of radium to therapy of human disease started empirically in 1901, when a Parisian physician named Danlos borrowed some radium from Pierre Curie and tried it on skin lesions. It was used sporadically, but not extensively, for superficial conditions until 1904, when Pierre Curie's observations on the greater sensitivity of diseased tissue gave impetus to attempts to treat malignancy. By 1905, radium plaques and implants were being used in New York and London, and intra-cavitary radium for carcinoma of the uterus was employed at Paris. It is interesting that interstitial placement of radium sources was first proposed in 1903 by Alexander Graham Bell, the inventor of the telephone. Viewing this new therapeutic method as an interested outsider (though no stranger to physics) he could see that external radium, as well as the weak x-ray apparatus of the period, could deliver little dosage to deep-seated tumors. It occurred to him that sealed sources could be buried in such lesions at the time of surgical exposure. This idea was expressed in a letter to a New York physician named Sowers.

Radium enjoyed an early advantage over x-ray therapy. Such a constant, trouble-free radiation source was in sharp contrast with the undependable gas tubes and the very rough control of their factors and dosage.

One feature of the early period was the systematic production of radon. Marie Curie herself supervised this activity from her own radium, and radon plants were set up in centers all over the world. By this means a modest quantity of radium could be "milked" for use over a considerable area. At first the applicators were of glass, but more substantial gold radon "seeds" for permanent implantation were developed by Failla in New York in 1926.

As more powerful x-ray therapy units came into use after 1920 radon plants were gradually shut down, though a very few still operate, and the use of radium itself became somewhat constricted.

With the opening of the Paris Radium Institute in 1914 (even though extensive work had to wait till after World War I) there was opportunity for Régaud to develop radium therapy under good conditions.

The other major centers at Radiumhemmet, Stockholm, under Heyman, and at Manchester, under Patterson, have followed. Refinement of technique was somewhat hampered by confusion in dosimetry, and it is only since the middle of the 1930's that we have had developed fairly precise methods (Quimby, Patterson and Parker) for delivering uniform patterns of radiation with dosages expressed in roentgens. The fact that even the best centers still vary as to technique simply indicates (a) that there is more than one way to skin a cat (i.e., get a good result), and (b) we don't know all the answers yet. Evaluation of cancer therapy is at best a very slow business.

During the past ten years radioactive cobalt has become generally available. This can be used in essentially the same fashion as radium, despite certain physical differences, the most notable being its 60 per cent higher output of gamma rays (13.6 r/min. for cobalt, 8.4 r/min. for radium). Considerable experience has been gained with this isotope since 1948, and it seems to be fairly well established as a competitor of radium. It will take quite a few more years to determine which, if either, is superior.

Most radium and cobalt have been, and are, used in the form of tubes and needles for application interstitially, in a plaque applied to a surface, or in various intra-cavitary containers. The commonest intracavitary use is in the uterus and vagina, but sources have also been placed in the rectum, bladder and esophagus. One additional recent (1951) adaptation of the interstitial technique is to put small cobalt or iridium wires in a hollow nylon suture which is laced through a tumor as a plane implant.

Large radium sources, up to five or more grams, were tried as teletherapy "bombs" or "packs" from 1918 until quite recently. These were interesting, as being comparable to supervoltage x-ray machines, but proved exceedingly expensive. The idea was good, however, and since 1951 cobalt teletherapy units have become increasingly popular, superseding radium in this area.

Radium was first produced commercially in Germany, by Giesel, who did a great deal to simplify the extraction process in 1901. He was followed in 1902 by Armet de Lisle in France, who worked closely with Marie Curie.

All radium was purified from the pitchblende deposits in Bohemia until about 1904. Then the Austrian government saw how valuable radium was becoming and clamped down an embargo. A world wide search for other sources was instituted, and faint traces of radium were found in many places. The best turned out to be a "carnotite" ore which had been discovered in Colorado in 1899. A good deal of this was shipped to Europe during the next few years, and by 1913—when the first production in the United States began—accounted for some 80 per cent of the world supply of radium at that time (about 30 grams). Carnotite was not a rich deposit; it required some 500 tons of ore for one gram of radium, as contrasted with five tons of pitchblende for a comparable yield.

In 1913, richer ore bodies in the Belgian Congo were discovered, but were kept secret during the war, and were only developed after 1920. From 1923 on this was the chief world source, and after 1926 American production ceased completely. However, there was a good deal of feeling between 1925 and 1929 that the Belgians kept the price artificially high, and there were recurring efforts, largely by Congressmen from Colorado and Utah, to revive an American industry. In 1930, good ores were found in Canada, so that this country is no longer dependent on transatlantic suppliers. As of 1953 (the latest information I could find), there were some 1500 grams of purified radium in use throughout the world. There is no problem of obsolescence, or wearing out, in the usual sense. Attrition through loss or theft is negligible. So, as long as there is any demand, the supply of radium will continue to rise.

During its first few years radium cost about $150,000 per gram. This had come down to $120,000 by the time American ores entered the market, and reached $70,000 with the development of the Congo mines. The price was down to $30,000 in 1930, and currently is about $23,000. In comparison, one curie of radio-active cobalt costs $3,300, but of course the upkeep on cobalt runs into money (about $250 per curie per year) because of its five year half-life and the need of replacement. As of 1960 purchase of cobalt and rental of radium can be considered competitive.

The history of radium would not be complete without mention of its use—and abuse—in internal medicine and industry.

To take the latter first: more than two thousand people were employed in Bloomfield, New Jersey, during the 1920's painting watch dials with radium. The industry used about two grams per year. The workers frequently licked their brushes to moisten them. Thus many of them managed to accumulate a good deal of radium in their bodies, most of it lodging in the bones. The forty-second fatality from cancer among these people was recently (1958) recorded, a woman of fifty-seven.

Radium preparations were also sold to women with the suggestion that they would, in some magic way, be enabled to watch their husbands at a distance.

As to medicine—radium and thorium were sold without restriction, and used as tonics for "what ails you" by doctors during the 1920's. The preparations were chiefly radium chloride, marketed as "radithor" or "radiumite" in solution or powder form. Radiumite was advertised as "nature's gift to mankind" (1903)—"it keeps you well when you carry it" in a chamois bag hung from the neck or girdle. It was used on cotton as a skin application, taken internally for arthritis, hypertension, anemia, etc., and inhaled, as radon, for tuberculosis and other ailments. By injection it was given intravenously or intramuscularly for many (or any) conditions. On the malignant side it was tried by injection for leukemia and lymphoma from 1904 on. A fascinating case was reported in 1942 (20): some 440 micrograms of radium chloride were given in seven doses intravenously over a six year period beginning in 1926 to a patient with Hodgkin's disease. Although he had some painful bone lesions (attributed to radium) in the mid-thirties, he was well and working in December, 1941, with no sign of the disease! Finally, phantom treatment crept in even here—a doctor was convicted in 1905 after claiming a radium cure when no radium was used!

Of course the quacks were on the scene too, as they are even today, peddling "Atomic-Nu-Life" and radio-active bath salts to restore youth. One "radio-active" gadget is advertised to cure cancer, diabetes, arthritis, etc. The same old snake oil in modern dress!

Nowadays, all this might seem pretty foolish, but one has to see things in historical perspective, and although enthusiasm may have gone too far, this is typical of medicine's trials and errors with innumerable drugs and methods over the centuries. A paper on internal radium therapy of arthritis was read at the fourth meeting of the American Radium Society in 1919. They had no idea of the dangers of radium within the body. The interesting thing now is that Robley Evans, at the Massachusetts Institute of Technology, and others, have been actively seeking out the survivors of the radium treatments and dial painting exposure, and have found the tolerance of these people to radiation surprisingly high. It may even be that our "maximum permissible" doses are too conservative. Another interesting investigation, currently in progress, has to do with study of the population of the Kerala district in India, where the natural background radiation level is up to forty times that in most parts of the world.

Now, where does radium stand today? It has been through periods of enthusiasm, abuse, and shadow—the rise of supervoltage x-ray machines has led some to suggest retiring radium completely.

Well, in teletherapy cobalt is able to compete on equal terms with supervoltage, and is in healthy condition. Here cobalt is treading in radium's footsteps, and sheds credit on its predecessor.

In therapy generally radium has found a firm place which is unlikely to be superseded as long as radiation remains a valid treatment. Where intensive sources at close quarters are needed and applicable— in the uterus, for instance—radium remains a method of choice.

Other Modes of Radiation Therapy

Therapy with the numerous radio-active sources (other than radium) now available must also be noted in this chapter. For the same reason which was cited under diagnosis this branch of therapy will be discussed under the heading of nuclear radiology.

Where does therapy stand now? Like roentgen diagnosis, it is firmly built into the structure of modern medicine. In cancer treatment it has nothing to fear from surgery—both are imperfect tools in this field, and operate best as partners, in team work. Any shift to chemotherapy of cancer is much more likely to come slowly than suddenly, and while that would reduce volume of radiotherapy there would always be advanced cases requiring palliative treatment. A major break-through in understanding of the causes of malignancy, leading to effective and preventive action, might more rapidly shrink our work. But there is also the very broad area of non-malignant treatment which will always be with us whatever happens with cancer.

On the administrative side, there is a growing tendency to separate roentgen therapy from diagnosis completely. This is already a fact in many big centers, where diagnostic and therapeutic departments may be almost autonomous. But you can't shrug off the provinces. There are still more people outside the big cities than in. At the small to moderate-sized general hospital in most towns the combined radiologist is still in demand and necessary. For while a man can usually make a living out of diagnosis alone, a full time therapist simply does not have enough work to support him outside the largest centers. The only way this might change is through an unlikely major upheaval in the way medicine is organized. For instance, regional health centers serving areas of 100-200 mile diameter (depending on population density) have something to be said for them, and might provide a way to make a full-time therapist available to the public. But things just aren't like that at present.

I cannot leave this discussion without giving as great emphasis as I can to this one point: despite all the isodose curves and physical formulas, therapy is very much an art (21, 22). It is an art because the precision of the treatment plan must always be applied to the extremely complex and variable biologic unit that is the patient. It is an art because the patient needs much more than roentgens of dosage—he needs experienced skill in optimum adaptation of the plan to him, he needs adjuvant hygiene and medical measures, he may need sedation, and—most important—he needs the reassurance and comfort that can only be given by a sincerely interested physician.

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Chapter 6

PROGRESS SINCE 1895—NON-MEDICAL APPLICATIONS

PHYSICIANS TEND, naturally enough, to view radiology as distinctly—or at least predominantly—medical. One might have had a right to do so during the first quarter of this century, but hardly since. I know that all of us are dimly aware of the wide uses of x-rays, radium, and other isotopes in fields outside of clinical medicine, but I doubt if most of us grasp the extent of such uses. Indeed, looking into the future, the day may not be far off when medicine will occupy a relatively minor place in the whole gamut of radiology—at least as regards volume. Volume, that is, both of work done and dollars involved.

Non-medical radiology got off to an early start (1, 2, 3). In Roentgen's first paper, of December 1895, he included a radiograph of a piece of metal. The reason for presenting this was to demonstrate what he called the "inhomogeneity" of its internal composition. Later he made radiographs of the lock of his own shotgun, carefully analyzing all the structures displayed by the differential densities recorded on the film.

Metallurgists could see the point at once: here was a method of examining almost any opaque object without tearing it apart. One of them remarked: "destructive testing methods are like striking a match merely to see if it is a good one." The war departments of Germany, Austria, and the United States were prompt to pick this up, and were using x-rays to examine cannon as early as 1896. Really large scale application of the idea was hampered by the limits of the equipment of the time. When radium became available a few years later it too was used for examination of metal objects. The United States Navy accumulated about fourteen grams of radium for this purpose. But the exposures required many hours, and when more powerful x-ray machines were developed radium almost disappeared from this field. Equipment capable of penetrating really large masses of metal came on the market in the early 1920's.

Prior to 1920, voltages were usually below 125 kilovolts, and pieces of steel thicker than 1½ inches could not be radiographed satisfactorily. In 1922, a 200 Kv, 5 milli-ampere unit was set up at the United States Army Arsenal, Watertown, Mass. On the basis of experience gained there the use and demand for x-ray inspection of metal objects grew rapidly and application of the method continues to expand. At present it is routine in most factories manufacturing metal castings, pipe, and so forth, to employ x-ray for detection of flaws. As a result, improvement of technique has cut down markedly the incidence of such faults as bubbles, cracks, and slag inclusions. Another related field is the inspection of welds, and this has led to important technical advances. In 1931, the Boiler Code of the American Society of Mechanical Engineers was changed to permit welded boilers for heaviest duty, if inspected by x-ray. The wide use of welding today is in considerable part due to making welds stronger and more reliable through x-ray monitoring.

The x-ray machine and film manufacturers count on these several types of industrial use for a significant proportion of their markets. I have tried to obtain some figures on this subject, but the field is so competitive that no one will say anything. All I could find were some tables in Census Bureau reports which indicated that in 1954, the value of x-ray apparatus sold was about eighty million dollars, and of x-ray film about sixty-three million dollars. This includes both medical and industrial, of course.

The details of metallurgical radiology are beyond the scope of this discussion, but—believe me—they are extremely highly developed, and the literature is large. A reference text (4) published in 1934, and revised for a second edition in 1943, contains a bibliography of 1,314 items.

Recently radio-isotopes have been infiltrating industrial radiography (cobalt, cesium, iridium), picking up where radium left off. They have the important advantages of portability, ease of application to awkward spots, and freedom from dependence on power sources when in the field.

Besides the large scale uses just mentioned there is an extremely varied field of other non-medical applications of x-rays. In most, the principle is the same one we use in medical diagnosis: analysis and interpretation of differential densities. Particularly valuable is the fact that the roentgen examination, whether it gives positive or negative information, is non-destructive. So often any alternative testing method does involve sacrifice of the item under investigation, or a part of it.

Naturally police, prison officials, mail inspectors, and customs officers have used x-rays to examine packages without tampering, for illegal radio-opaque material, bombs, and contraband. The story is told of one zealous American official during World War I, who finished examining a shipment of cotton bales, and went on to expose to x-radiation a consignment of photographic film.

Legal documents, particularly parchment, have often been subjected to roentgen study, and the results have been important in litigation. Erasures, substitutions, or other improper tinkering may be detected by the infinitesimal alterations in thickness recorded by the x-ray. Similarly, in the manufacture of many items, thickness and size can be precisely monitored.

Oil paintings lend themselves to this sort of examination, though here the densities of the inorganic pigments are usually most significant. Overpainting and alterations are often important in analyzing a given work, and such items may play a large role in establishing authenticity. I know personally a gentleman who claims to be convinced on the basis of x-ray evidence that a painting which is accepted as the work of a famous old master is a later copy. He says he has been frightened out of testifying by threats of reprisal from an art dealer concerned. It seems likely that there must be evidence both ways. But even if this story sounds improbable I submit it merely to point out that large amounts of money and important facts may be involved in controversies where x-ray evidence plays a part.

X-rays of antique metal objects, coupled with a detailed knowledge of history—just as we must bring our background of pathology and clinical medicine to bear in diagnosis—have revealed many identifying and dating data not otherwise available. In 1692, the city of Port Royal, Jamaica, sank during an earthquake. A watch was brought to the surface by an expedition which was searching for relics in 1959, but was completely encrusted with lime and barnacles. Without damaging it in the least, x-rays showed the exact hour at which the hands stopped, indicating the time of the first violent shock.

Dozens of commercially used items made of metals, wood, ceramic, plastic, and rubber have been studied for flaws, impurities, or foreign material, just like industrial castings. The list is endless, and if one looks for them more keep turning up all the time. The following are a few specific examples:

Electric and telephone wires; wire rope.
Heavy grindstones, up to ten inches thick.
Reinforced concrete for homogeneity and position of rods, after settling.
Fine woods, to detect knots, resin pockets, worm holes.
Standing timber, to select for special uses.
Fire-clay pots, used in glass manufacture, for flaws.
Porcelain, for analysis of quality, impurities.
Mica, for impurities.

Shipments of various grains, foods, fruits, nuts, chewing tobacco, etc. (One candy manufacturer installed x-ray monitoring after being sued by a person who swallowed a piece of wire accidentally included in one of his product's boxes.)

The following are some uses of x-ray to discover data hidden to other methods:

Detection of gold in quartz.
Metal vacuum tubes, for correct position and alignment of grid and filaments.
Tooth paste tubes, for uniformity of tube filling.
Beer cans, for uniformity of filling.
Fluid systems, for air pockets.
Search for hidden pipes and wiring in old buildings.
Height of a liquid column in metal pipe.
Analysis of gems, for determining quality and genuineness; e.g., natural salt water pearls fluoresce under x-ray, cultured ones do not.
Study of Egyptian and other mummies; "Paleo-pathology" of bones.

X-ray spectroscopy is a special technique with many applications. With it qualitative and quantitative analysis of extremely small quantities of material are possible. Half a dozen new elements have been identified by this means. Industrial applications include:

Measurement of concentration of a chemical in solution, e.g., tetra-ethyl lead in gasoline.
Composition of alloys, including detection of counterfeit coins.
Distribution of impregnating agents in wood.
Determination of porosity of materials.

X-ray diffraction is yet another method, which at first may seem to be of merely theoretical interest (4, 5). But the ultimate object of diffraction studies is understanding the fine structure of matter. And there are many practical applications of such understanding. These include improvement in design and processing of special steels, structural materials, natural fibers, and synthetic polymers. It has also been possible to study the effects on fine structure—be it crystalline, fibrous, colloid, or amorphous—of strain and mechanical deformation, with resulting improvements in durability and safety.

Biological investigations not infrequently employ radiography. I recently came across an article describing the use of x-rays to study the details of mollusk shells. There have been many other biological applications of radiation, of course. In research the experiments on Drosophila mutations over thirty years ago have cast a long shadow down to the present. Sterilization of foodstuffs, drugs, and sutures has been carried out on a practical scale.

One very special biological use came to my notice recently (6). It is rather ingenious. It seems that the screwworm, which is really a type of fly, is quite a cattle plague. The female lays eggs in surface wounds and scratches, and the maggots literally eat the animal up. A heavily infested steer can be killed in ten days. This pest had been doing a great deal of damage in Florida cattle herds. Well, it seems that the female mates only once. If she mates with a sterile male, all her eggs thenceforth are no good. So the Department of Agriculture bred screwworm flies wholesale, exposed them to enough gamma rays to sterilize (but not damage otherwise), and a few years ago scattered them over Florida by plane. It worked; the screwworm population was practically wiped out.

Not all attempts to use x-rays are successful. I have been told that about forty years ago someone tried (presumably on the theory of producing chemical changes) irradiating fresh whiskey containing oak chips in order to accelerate aging. This did not work. The idea dies hard, however. In 1959, "ultrasonic" waves were being suggested as a possible means to age wine artificially.

Some applications are even less scientific. Fluoroscopes in shoe stores belong here, and of course may be actually harmful. Another shoe story is this: An unscrupulous salesman exhibited a radiograph of a pair of ordinary shoes, to show the many wicked looking (and at least by implication, harmful) nails. Then he showed a film of the brand he was peddling, whose soles were sewn, indicating that they were better and safer on that account.

From the foregoing you may form some idea of the extent and breadth of non-medical radiology—just with x-rays and radium. The volume and scope are tremendously increased when we add the non-medical uses of radioisotopes. This extensive field will be discussed in the chapter on nuclear radiology.

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Chapter 7

NUCLEAR RADIOLOGY

TO CONSIDER RADIO-ACTIVE isotopes separately is perhaps arbitrary. After all, they are sources of ionizing radiation, and their application extends over the gamut of diagnostic, therapeutic, and non-medical radiology. But they form a coherent historical unit. Furthermore, the techniques are becoming so specialized that nuclear medicine bids fair to set itself up as a semi-autonomous subspecialty. Meanwhile radiologists should familiarize themselves with it and take it under their wing—unless one wishes to see a department of medicine or pathology take it over. In a few very large institutions completely independent departments of nuclear medicine have already been established.

Isotopes have been recognized as such since the work of Soddy in 1910. We all know that they are chemically identical variants of the same element, differing only in atomic weight due to differences in the number of nuclear neutrons.

Natural radio-active isotopes are as old as the world, though of course they have only been known about since 1896. However, it is the newer, plentiful, and fairly cheap artificial ones which have made the subject as important as it is today. For though certain isotope uses merely duplicate or improve upon those of x-rays and radium, it is the tricks of tracer technique and selective localization which have added new dimensions to radiology during the last twenty years.

It was the cyclotron, developed by Lawrence in 1932, which first made radio-active isotopes available in quantity. Nuclear reactors, utilizing uranium fission, provided even better and less expensive production of many isotopes after 1942. In 1957, only twelve years after it had put them on the market, the United States Atomic Energy Commission made 14,126 isotope shipments from Oak Ridge alone. Three hundred eighty-three of these went abroad.

We have already seen how intimately isotopes have been involved in the development of physics in this century. We are not going to talk about that any more. Nor can we do more than note in passing the tremendous volume of research work in many diverse fields where isotopes are integral to the methods. We are just going to review the ways in which radio-isotopes are put to practical use, and how those uses developed.

If you wish to get some idea of the scope of nuclear research, just glance through the four thick volumes titled *Radio-isotopes in Scientific Research* (Edited by R. C. Exterman, Pergamon Press, New York, 1958) which report papers given at an international conference devoted to such research in Paris, September, 1957, under UNESCO auspices. Other such international meetings will certainly result in more heavy volumes.

As to instruments, the Geiger counter with its associated amplification apparatus has been the chief tool for registering radioactivity. It was based on principles developed in 1906, and greatly improved in 1928. Prior to it the photographic film was the only recording device. Within the last few years scintillation counters have provided a greatly increased sensitivity (permitting lower dosage in diagnostic techniques) and selectivity of energy ranges.

MEDICAL APPLICATIONS

The first artificial radio-isotope made available to the public was phosphorus (P-32) in 1945. But this was only after some ten years of research effort.

Of course, natural ones were available earlier, and thorium-B (Pb-212) had been used as a radio-active tracer in studies on plant physiology by Hevesy in 1923.

A less formal, but perhaps equally significant tracer experiment with a natural radio-isotope was actually performed about ten years earlier, in 1912, by the same Hevesy. At that time he was working in Rutherford's laboratory, in England. He became suspicious of the length of service of the roasts served at his boarding house. Using radioactive lead he contrived to label at their first appearance some slices of the meat, and left these on his plate. Testing the ragouts and stews over the next week showed the radioactivity coming back to table day after day (5).

The first clinical use of a stable isotope was by Hevesy and Hofer in 1934, with deuterium. By a combination of tracer and dilution techniques they used it to study human water metabolism.

In 1935, an artificial radio-isotope was first put to work on a biological problem by Chiewitz and Hevesy, investigating the distribution and excretion of phosphorus in rats.

Human application of radio-isotopes occurred first in 1936. Studies on the metabolism of cyclotron-produced sodium were conducted in March by Hamilton and Stone, and in December phosphorus was first tried in the treatment of leukemia by Lawrence.

Radio-iodine has been studied in animals since 1938 (Hertz, Roberts, and Evans), and in humans since 1939 (Hamilton and Soley; Leblond and Sue). Hyperthyroidism was first treated by radio-iodine in 1942 (Hertz and Roberts; Hamilton and Lawrence), and thyroid carcinoma in 1943 (Seidlin, Marinelli and Oshray).

So much for the pioneers. Obviously, only a few of the major "firsts" can be mentioned.

The following list of radio-isotope uses is by no means complete. Many special methods have not yet progressed beyond the experimental stage, and newer ones are being developed all the time. On the other hand, some of those listed may in the course of time be abandoned or superseded. The applications which account for the greatest volume of medical isotope work at this time are marked with an asterisk.

I. *Diagnosis*

Although all the following examples (a) through (d) employ the tracer or labelling method, they may conveniently be divided into several sub-groups.

(a) *Dilution Techniques*

1. Red cell survival studies (iron, chromium).
2. Red cell mass (iron, chromium phosphorus).

*3. Plasma volume, with radio-iodinated serum albumen—"RISA" (iodine).
4. Ascitic fluid volume, with "RISA" (iodine).
5. Total exchangeable body sodium and potassium.

(b) *Physiological Localization Techniques*

*1. Thyroid gland function: uptake, clearance, protein-bound iodine (iodine).
*2. Mapping normal, enlarged, aberrant, metastatic thyroid tissue (iodine).
3. Localization of tumors—e.g., brain (iodine, phosphorus, potassium, copper, arsenic), breast (phosphorus, potassium), bone (gallium), testis (phosphorus), eye (phosphorus), as well as mapping their shape and size, and their metastases.
4. Gall bladder function, with di-iodofluorescein (iodine).
5. Kidney function, with diodrast (iodine).
6. Liver function, with rose bengal (iodine).
7. Radio-autography, recording on film the amount and distribution of a radio-active substance deposited in tissue.

(c) *Flow and Diffusion Measurements*

1. Blood velocity and circulation time (first studied by Blumgart, Yens, and Weiss in 1924, using naturally radio-active radium-C deposited on sodium chloride).
2. Study of intracardiac shunts (sodium, krypton).
3. Study of adequacy of circulation in pedicle grafts (sodium, phosphorus).
4. Study of adequacy of femoral head blood supply (phosphorus).
5. Study of cutaneous burn depth; partial or full thickness (phosphorus).

(d) *Study of Organic Metabolites*

Analysis of biochemical processes—routes, transformations, storage, excretion of: sugars, proteins, phospholipids, vitamins, hormones, fatty and amino acids, cholesterol, etc. Much of this is of a research nature, but has led directly to practical diagnostic tests, e.g.:

- *1. Test for pernicious anemia with cobalt-labelled vitamin B-12.
- 2. Test for pancreatic insufficiency with "RISA."

(e) *Radiography*

This is still in the experimental stage, and restricted to special conditions, (e.g., military) where electric power is unavailable (thulium, cesium).

II. *Therapy*

Here the mechanisms vary, and will be indicated. In each case the object is to deliver an effective dose of ionizing radiation to the susceptible tissue or tumor, while sparing other tissues as much as possible.

(a) *Physical Placement*

- *1. Radium, radon and cobalt tubes and needles in familiar external, intracavitary, and interstitial applicators. Recently yttrium pellets have also been used.
- *2. Strontium applicators, as beta sources, in opthalmology.
- *3. Serous surface application: colloidal zinc was first used in 1945, gold since 1947.
- 4. Direct infiltration of fluid or colloid suspension. Gold was used for prostatic carcinoma first in 1951. Parametrial injection has also been tried, as part of the treatment of carcinoma of the cervix uteri.
- 5. Endobronchial application. Silver and gold for beta irradiation of nodes.
- 6. Intrathecal application. Colloidal gold for control of medulloblastoma or other tumor.

(b) *Teletherapy*

- 1. Radium (first used in 1918).
- *2. Cobalt—the principal teletherapy source at this time.
- 3. Cesium—this element is of great interest because its thirty-three year half-life makes replacement less of a problem than with cobalt. Further, its gamma rays are less penetrating (1 Mev), thus requiring less shielding. Cesium is still not sufficiently plentiful or cheap to be extensively developed.

4. Neutron beam—this was first tested in 1935. Recently it has been used to irradiate boron selectively deposited in a tumor and make it radioactive, giving off alpha rays for local therapy. This is still experimental.
5. Proton beam—still experimental, but it has been used to knock out the pituitary gland in selected cases.
6. Electron beam—from the betatron, still experimental.

(c) *Biochemical Placement*

*1. Iodine for reducing thyroid gland activity:
    (i) Hyperthyroidism. It is interesting that here the wheel has come full circle. X-ray therapy rose to a prominent place in the treatment of hyperthyroidism in the early 1930's, but gradually fell into disuse as surgery and anesthesia were refined. Now we have the spectacle of surgery steadily shrinking its rôle, while ionizing radiation—perhaps more effectively delivered—takes the lead.
    (ii) Ablation of the normal thyroid as part of the treatment of thyroid carcinoma.
    (iii) Cardio-vascular disease: intractable angina pectoris, congestive failure, cor pulmonale, intermittent claudication. The rationale of this sort of treatment (by surgical means) had been proposed for heart disease in 1924.
    (iv) Intractable Parkinsonism.
*2. Iodine for irradiation of tumor in functioning thyroid carcinoma, both primary and metastatic.
3. Phosphorus for treatment of leukemia.
4. Phosphorus for treatment of polycythemia vera.

See how wide and diverse are these medical uses! It is interesting that (apart from radium) only four isotopes account for an overwhelming proportion of medical work: iodine, phosphorus, cobalt, and gold. It is also of interest that procedures for diagnosis and treatment of neoplastic diseases are greatly outnumbered by applications to benign conditions. If we consider volume of tests and treatments, malignancy possibly accounts for about one-third.

Early enthusiasm for isotopes as a major contribution to cancer therapy has been rather dampened to date. Still, even though the most rapid advances seem to be elsewhere at present, the picture could change dramatically at almost any moment.

Radio-isotope dosimetry is not satisfactorily precise. Except where the rules developed for radium can be applied, how much of an internally administered isotope gets where and for how long is most difficult to control and measure. Thus therapeutic doses are usually arrived at by a combination of facts and "judgment," the latter representing experience and educated guessing. In diagnostic work, however, the aim is to keep radiation exposure to a minimum. This is simply a matter of using the least amount which will do the job and keep the dose well below accepted permissible limits. In general, this has not been a problem.

The large number of miscellaneous medical uses accounts for the acute interest of clinicians in the field, and this will certainly keep on growing. New isotope techniques continue to multiply. And so does the number of doctors using them; there were in 1959, about 2000 licensees. Obviously, nuclear medicine is here to stay.

NON-MEDICAL APPLICATIONS

The title of this chapter, Nuclear Radiology, was deliberately designed to indicate that the field is a great deal wider than just nuclear medicine. How much wider we will now see. Isotopes can do most of the things x-rays can do, as well or better, plus a tremendous amount more.

I. *Explosions* (from the uranium fission chain reaction and from nuclear fusion)

   (a) *Military Weapons*:

   Here radiation is a by-product, and an undesirable one except for whatever value it might have in inspiring terror. It is too inadequately controllable to be a useful instrument of war, in any event. From the military standpoint, it is the enormous blast and heat which are the immediately effective results of the explosion.

   Considerable effort is being put into development of relatively "clean" bombs.

(b) *Peaceful Uses*:

Military explosions were cited first merely because they came first chronologically. Other uses are potentially tremendous and will certainly come. Let us remember that Alfred Nobel invented dynamite for peaceful purposes. Here are a few possibilities recently suggested by Edward Teller. These all involve underground explosions so that radio-activity is confined and does not present a problem.

(1) Excavation of harbors, waterways and irrigation chanchannels. Uncovering deep mineral deposits for open-pit mining.

(2) Breaking up impervious rock formations to allow rivers flowing near or through deserts to penetrate the soil. Large barren areas could thus be made productive.

(3) Heat of underground blasts could be drawn upon for power production (which we will come to presently) or used to drive oil out of shale and tar sands. Natural volcanic heat has long been put to industrial use in Italy and New Zealand.

II. *Heat and Power*

(a) *Space Heating*

Conversion of nuclear energy to heat is not difficult. In fact this heat, which is incidental to certain reactions, is actually used for space heating at some Atomic Energy Commission installations rather than let it go to waste. At present, this method cannot compete in areas where conventional fuels for heating are plentiful; but that is not to say that this will always be so. Even now nuclear heat would be applicable to remote places. For instance, at the time of writing, 75 per cent of all cargoes flown to United States outposts in Antarctica consist of fuel oil. Imagine how expensive that is!

(b) *Electric Power*

Conversion of nuclear energy to electric power is already feasible; the first such electricity was produced in an Atom-

> ic Energy Commission plant in December, 1951. This is done by heat transfer, driving generators by steam. In 1959, electricity produced by conventional means cost seven to eight mills per Kilowatt Hour; at the Shippingport, Pennsylvania, reactor it was 64 mills per Kilowatt Hour. However, fuel is only 15 to 20 per cent of the cost of electricity, so scope for improvement is not too great. Still, experts believe that the cost of conventional electricity will rise over the years, while that of reactor-produced electricity will fall. Looked at from another standpoint, the economy of nuclear fuel can be terrific: one ton of uranium will do the work of ten thousand tons of coal. And with improvements this ratio may be increased as much as six times.

So much for the United States. Some areas abroad, where fuel costs are much higher than here, are already ripe for nuclear power. In fact, they are clamoring for it, even though the technology involved is still incompletely developed. Several different methods of power production are right now being tested, and there is considerable controversy over which may turn out to be the best. As of 1959, the so called "boiling water" reactor seemed to be leading the pack. In England, research is advancing very fast too, and experts there say that by 1963, nuclear power will cost no more than conventional.

An exciting possibility for the future is that energy from nuclear fusion may be converted to electricity directly, without requiring heat transfer mechanisms. This would be a momentous advance. The prime fuel for fusion, deuterium, is fairly plentiful and comparatively inexpensive. Furthermore, fusion does not produce the radioactive by-products which the fission process does; these are undesirable wastes and expensive to cope with from the standpoint of the power producer.

Away from fixed electrical outlets batteries have a special value. An experimental nuclear battery has already been made which lasts many times longer than chemical ones, and is much lighter. It uses heat from a polonium source, converted to electricity by conventional thermo-couples. A very significant new development was reported early in 1959. It is a "plasma thermocouple" where electricity is produced as a result of the heat derived from fission of uranium bombarded by neutrons while in contact with cesium.

(c) *Propulsion*

If we can extract heat from nuclear reactions, and with it set wheels turning to generate electricity, it isn't too big a step farther to make the same wheels do mechanical work of some sort. The problems of making the unit portable and safe were not too great.

Understandably, the expense involved makes nuclear propulsion somewhat remote for large scale public use. But military services are not restrained by the same economic strictures, and one obvious place where improvement of the existing propulsion methods would be welcomed is in submarines. Getting rid of dependence on storage batteries which severely limit duration of dives would be a big advantage. So would getting rid of bulky oil tanks and diesel engines. This has been done, of course. The Nautilus, first atomic submarine, was launched in January, 1954, and has been followed by others. The Navy plans quite a few more, as well as several surface craft. Meanwhile, the Russians are known to have at least one nuclear vessel, an ice-breaker, in operation. The keel of the Savannah, first nuclear merchantman, was laid in May, 1958.

A lot of work is being done to adapt nuclear power to aircraft engines. Weight of the shielding necessary for personnel safety is one of the biggest difficulties. But it is just a matter of time until all of these problems are solved.

III. *Industry*

Unlike the three subjects just covered, industrial use of isotopes is quite well developed and operating economically. In fact, it is estimated that isotope techniques saved American industry about 500 million dollars in 1957 and 1958. Optimistic forecasts suggest that annual savings may reach five billion in ten years. Here are some of the methods employed by over 1700 installations.

(a) *Radiography,* principally with cobalt, is a coming thing in industry. I have touched on it briefly already, but it deserves mention again because of its importance. Besides the large fixed installations, small portable units using thulium or iridium are also available.

(b) *Thickness gauges,* for every sort of material. For instance, ninety per cent of all tire fabrics and eighty per cent of all tin cans in this country are thus tested. The precision of the system makes it possible to do such jobs as monitoring plastic film only 5/10,000 inch thick.

(c) *Density measurements* can be applied to almost anything. One practical way is to determine the density, and hence the firmness, of sites for aircraft runways, roadbeds, or such massive objects as dams.

(d) *Tracer techniques* are innumerable. Enthusiasts have said that this tool is the most significant development since the invention of the microscope. The following are a few examples:

1. A tracer introduced into an oil pipe line will indicate the end point of one grade, and change to another.
2. A tracer in a buried or inaccessible piping system will indicate the site of a leak, and so minimize the damage necessary for repair.
3. Wear of piston rings is measured by rate of accumulation of radioactivity in crankcase oil; the metal of the ring was made radioactive beforehand. Similar techniques measure wear of tire treads, ball bearings, and many other objects.
4. Analysis of petroleum refining procedures with tracers has led to significant improvements in gasoline.
5. Assay of vitamins D and B-12 using tracers has replaced cumbersome biological tests. The assay is very easy, rapid, and much more accurate. As with most of the other examples cited, the resultant savings are immense.
6. Separation of reserpine from similar rauwolfia compounds was extremely difficult until study of the behaviour of reserpine labelled with a tracer made possible a practical purification system. Tracer technique has also shed a lot of light on the metabolism of this drug, which is effective in such small doses that more classical methods could not be applied. These types of study are

representative of literally hundreds of "process control" methods being used more and more widely.

7. Studies on tin fluoride in a dentrifice, using a tracer, established the fact that fluoride was deposited on the teeth. This is obviously interesting both scientifically and economically.

(e) *Effects on chemical bonds* have resulted in many interesting and valuable effects, such as polymers not otherwise feasible, new plastic materials, "grafting" one plastic layer onto another, and new ion exchange membranes.

(f) *Neutron activation analysis* has proved particularly useful in identifying and measuring precisely extremely minute traces of elements. Amounts as small as 10^{-16} grams can be quantitated. In a recent brochure put out by one of the companies making isotope equipment, some twenty-six different applications of this method were cited.

(g) *Sterilization* of drugs, sutures, and surgical dressings is valuable wherever chemical or heat sterilization is undesirable or more expensive.

IV. *Agriculture*

Agriculture is another very important realm for nuclear radiology. It is difficult to say whether or not it is second to industry in magnitude of applications. Indeed, agriculture *is* industry, in a sense. It is also biology, and (veterinary) medicine. Again we are going to pass over the vast amount of basic research in many phases of this field, and merely mention some practical uses.

(a) Experimental studies of radiation-induced mutations have resulted in hardier and more disease-resistant grains. Even though only a small proportion of such mutations may be beneficial, the ones which are can do wonders in increasing yield and quality. Tracer study of plant nutrition has also contributed a great deal.

(b) Slowing down biologic activity may have practical value. By means of radiation potatoes can be prevented from sprouting, thus making them marketable for a longer time.

(c) Sterilization of such things as surgical supplies has been mentioned. Similarly, with an intense beam of radiation, sterilization of foodstuffs can be accomplished, making indefinite storage possible.

(d) Work with tracers on metabolism of various feeds in dairy cattle has shown the way to adjusting their diet to result in considerably improved butterfat percentage of milk. This is a matter of hard dollars and cents to the farmer, whose milk price is directly related to butterfat content. Studies of the same general type have led to improved nutrition of the animals themselves, with economic benefits resulting from their health, and also in meat production.

(e) Many factors besides feed go into making milk. To a considerable extent empirical breeding of the best stock has been the only way of improving yield in the past. This takes years, of course. Tracer techniques have speeded up some aspects of this process. For instance, optimum thyroid function is related to milk production. Nowadays calves which are deficient in this regard can be identified with iodine tracer studies, and be eliminated early, rather than waste time and feed on them for two years before finding out that they are poor milk producers.

(f) Grafting of trees and plants is an art which has been practised empirically for centuries. Tracer methods are now telling us a great deal about transport of nutrients across the site of union, and are guiding us to better and more productive grafting techniques.

V. *Archeology* (10)

Radio-active carbon (C-14) is a pure beta emitter with a half life of 5568 years. It has been used for a number of investigations on drugs and organic compounds, but to date has found no real medical or commercial utility. Still, since carbon is so ubiquitous in biologic chemistry, one would not be at all surprised to find C-14 important in medicine or agriculture one of these days.

However, it is in the fields of archeology and geology that radioactive carbon has made a really significant mark. How does one account for the natural occurrence of a radio-element of such low atomic number and weight as carbon? In 1946, W. F. Libby came up with an explanation which has won general acceptance: neutrons set free from collisions between cosmic rays and gas molecules in the earth's atmosphere over eons of time bombard nitrogen-14, changing it to carbon-14. He went on to observe that such radio-active carbon in organic material should decay at a constant rate following the death of the organism of which it was a part. From this it follows that a decay curve can be plotted, based on the carbon-14 measured in the specimen now. By comparison with historical events of known age, a remarkably accurate dating has been developed. The chief archeological use concerns placing in correct chronology various pre-historic relics of human cultures.

In geology, where time is considered on a much grander scale, dating methods up to now have been relatively crude: stratigraphic layering, evidence of successive glacial periods, and paleontologic evidence of fossil development. Radio-carbon has introduced a large increase in accuracy—one is almost tempted to say precision, relatively. Geologists have been having a field day. They are taking measurements of any and every specimen that comes to hand. The literature is very confusing to an uninitiated person like myself, and I dare say that this field will pass through a data-gathering phase for several years before any new laws of nature emerge.

V. *Meterology*

Study of the movement of ocean currents and air masses, particularly in the higher levels of the atmosphere, can be conducted with isotope tracer techniques. Information is being gathered steadily, and the heady suggestion has been made that when we have nuclear power under more complete control we will have the wherewithal to compete with the sun in influencing our weather.

VII. *Miscellaneous*

Below are listed a few tracer applications which do not seem to fit any of the above categories.

(a) *Biochemical research*
   1. Study of intracellular reactions.
   2. Study of protein structure.

(b) *Geographical studies*

1. Tracing movement of silt in rivers, estuaries, harbors.
2. Tracing water flow in rivers, sewers, underground seams and channels.

(c) *Identification*

1. A minimal amount of long-lived tracer substance, built into paper money, could distinguish it from counterfeit.
2. Any other material could be so labelled to establish authenticity.
3. Dangerous concentrations of toxic gases in laboratories and factories can be measured extremely sensitively—more so than in any other way—by chemical reaction. For instance, sulfur dioxide will react with labelled chlorine; even a tiny change in concentration of the latter can be recorded and used as a danger signal.

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Chapter 8

RADIATION HAZARD AND PROTECTION

I DO NOT PROPOSE to discuss in any complete fashion the complex matter of radiation protection. The basic facts are in reference sources, but one must also keep a close eye on the literature streaming by in order not to lose contact with new developments. The April, 1959, issue of the journal *Radiology* contained a collection of good articles which brought the topic up-to-date as of then. These discussed the matter from the several viewpoints of the radiologist, the physicist, the geneticist, and (not least) the government. What I want to do now is just to touch on the chief historical foci of the subject.

I have already mentioned, in a general way, the gradual awakening to the dangers of radiation that went on during the early years. Unfortunately, the lag between recognition of an evil and action to correct it has been particularly long drawn out in this case. How many needless hand injuries were there between 1900 and 1910? Voices were raised to condemn the promiscuous use of the hand to gauge penetration even then. Nowadays young doctors are emerging into radiologic practice with a keen awareness of the radiation hazard, and we may expect that most will keep it to a minimum.

My optimism suffers a set-back, however, when I see that thorotrast, which has been roundly condemned by radiologists and the American Medical Association, is still being advertised for sale, and literature within the past ten years continues to recommend it as an angiographic contrast medium. Nor can one be happy about the unskilled use of x-rays by many non-radiologists.

These matters of who is qualified to do what, and the safety of our tools and drugs are very important aspects of the protection issue. They prompt questions, such as these: 1) Should thorotrast be sold at all? 2) Should persons who have not had special training in radiology be permitted to own and/or operate x-ray apparatus? 3) Is roentgenographic interpretation or radiation therapy by an untrained physician any less improper than surgery performed by an incompetent person? 4) If one has to demonstrate experience and competence in order to get permission from the Atomic Energy Commission to give a tracer dose of radio-iodine, why can anyone who desires to buy radium do so, and do whatever he wants with it?

These, and many other similar questions are going to be around for a while, and answers are going to have to be found.

Radiation hazard and protection were rather small matters—from the broad public health viewpoint—so long as only x-ray machines and radium were concerned. But now that the age of nuclear radiology is upon us, everybody is affected, and it becomes a subject of major public health importance. A spasm of acute public alarm was precipitated by the 1956 report of the National Academy of Science. This told nothing that was new to radiologists or physicists, but to the public it was shocking. The violence of that paroxysm (which seems largely to have spent itself in a few years) was itself surprising to me. But presently I realized that people react much more strongly to discovery of danger where none was imagined than where it is implicit. It is as though one were betrayed by a trusted friend. Nobody gets very startled if someone falls through the ice while skating—distressing to be sure, but that was the risk he took.

That brings me to the concept of the accepted risk. We take for granted that there is risk attached to everything—even living. But certain things get to be considered more free of danger than others, and we live out our allotted spans because we do avoid, day-in, day-out, falling prey to the chance of injury or death from any of the dozens of things we do. Getting back to medicine, we accept calmly the idea of a certain per cent mortality from such and such a surgical operation, and the predictable number of reactions from such and such a drug. But radiation has been considered innocuous for so long! It bothers us to think that a radiograph today may be responsible for a stillbirth three generations hence.

Maybe it is the uncertainty, and the remoteness of cause from effect (even when they can be connected), which make the situation more anxiety-producing than such a simple relation as: one hundred surgical operations, ten deaths.

Well, I suggest that we face the facts squarely, Can ionizing radiation be dispensed with? Emphatically, no. The military won't give it up; industry won't give it up; medicine won't give it up. Absolute protection from radiation is therefore out of the question. Certainly we will minimize exposure, but there is bound to be some damage. This is the risk we accept. Maybe a few people will die, maybe some of us will live a little less long, maybe some of us will get leukemia—we do not know. But aren't the benefits of radiation worth it? Certainly they are, and I think the value we get from radiation compares very favorably with the value we get from automobiles, which cost the United States alone some forty thousand deaths every year. There is a large number of dangerous and harmful things in our environment—each of you could name half a dozen (e.g., tobacco, fire, liquor, fire-arms, dynamite, industrial air pollution) —which we tolerate because they give us value despite their risk. Even the beneficent antibiotic Penicillin has killed well over one thousand people. Radiation must simply take its place among these. We (and I mean everybody) must learn to accept the risks coolly. We can even take pride in the remarkable safety record of the United States Atomic Energy Commission: out of thousands of employees working with or near nuclear reactors between 1942 and the time of this writing, only seventy-seven were accidently exposed to excessive amounts of radiation, and only three died. Looked at in perspective, the risks of radiation are really rather small.

Now if we accept the risks, we can move on to consider what they are, and what to do about them. A good deal is known about acute and high level radiation exposure. The results are prompt. It is the low level exposure which is received by persons working with x-rays or radio-activity, and by the general public as fall-out from nuclear explosions which is the present and future problem. Our knowledge of this area is scanty but growing. Some aspects of it are pretty grim; for instance, many foods show increasing amounts of radio-activity, and human bones, particularly children's bones, reveal increasing deposits of radio-strontium. As of this moment, we do not know exactly where we stand in regard to the risk to mankind of receiving steady low-level radiation from elevation of the background. Whether or not such heightened exposure will result in any shortening of life span, increased incidence of leukemia, or remote genetic effects is another important question for the future. Perhaps it may not be as serious a problem as overpopulation of the globe by natural increase.

The concept of a tolerance dose, or "maximum permitted exposure" has developed rather slowly, and until very recently has been consistently linked with the idea of a dose which will avoid *any* damage. As I have suggested above, this is gradually being redefined as the dose which will give minimum risk while getting the work done. If you object that this is no definition, all I can say is that it will have to do; it is just as good as the definition Abraham Lincoln gave for the proper length of a man's legs—"long enough to reach the ground."

The first reference to human tolerance for radiation was by Rollins, in 1902. He said, "If a photographic plate is not fogged in seven minutes, the radiation is not of a harmful intensity." Just what was the basis of this opinion is not clear, since depressive effects on the peripheral blood, on bone growth, and on spermatozoa were not demonstrated until 1903.

In 1915, the Roentgen Society (London) published a manual of "Recommendations for the Protection of X-ray Operators," but this included no mention of a permissible level of exposure.

In 1925, Mutscheller, of New York City, went at the problem fairly systematically, observing the level of radiation in places where personnel had been working for a long time with no detectable ill effects. He came up with a figure of 0.01 erythema dose in thirty days (200 working hours) as safe for indefinite exposure. This has since been estimated to be close to 0.2 r/day.

Also in 1925 and 1926, Glocker and Knaup carried out a parallel investigation, in Germany, and came up with a not greatly dissimilar figure of 0.015 erythema dose per working month.

In 1928, Barclay and Cox summed up the literature for the previous several years and added their experience in England. Their recommendation reduced the "safe" limit to 0.0084 erythema dose in a month.

In 1932, Failla, investigating radium in much the same way in the United States, made a recommendation of 0.1 r/day. This figure was adopted by the United States Advisory Committee on X-ray and Radium Protection in 1936. The International Committee on X-ray and Radium Protection had endorsed 0.2 r/day in 1934. These two bodies have usually been pretty close together, but do not always meet and make announcements simultaneously.

After World War II, because of the vastly increased number of people occupationally involved with radiation, and the mass of records accumulated on them, a general revision of all safety standards was undertaken. All of the data on threshold doses for minimum effects on hematopoiesis, leukemia and other carcinogenesis, aplastic anemia, reduced fertility, reduced life span, etc. were studied. The upshot was the maximum permissible exposure of 1948 (National Committee on Radiation Protection, United States): 0.3 r/week for total body or gonads, and 1.5 r/week for the hands. The International Committee made the same recommendation in 1950.

In1958, a further downward revision was announced by the National Committee on Radiation Protection: three rem per quarter year (0.23 rem/week) and double that for the skin of the hands. The reasons cited for this latest reduction were: further to minimize possible genetic damage and possible life shortening. Note that there is no mention of damage due to the previous maximum permissible exposure; there was none. In fact, *nobody, observing the currently accepted maximum permissible exposure, has ever sustained any radiation damage to himself* (e.g., leukemia)—except for possible unrecognized life shortening. On this subject of life-expectancy there has been much argument, but little in the way of concrete fact. Two fairly recent papers are of interest. In 1958, Court-Brown and Doll (7) reported a survey of British radiologists since 1897, and concluded that there was no convincing evidence of radiation-induced life-shortening, at least among those entering radiology since 1920. Before that date there was a slightly higher incidence among radiologists of skin cancer, pancreatic tumors, and leukemia. In 1961, Henry (8) reported interesting observations on animals indicating that below a certain level (approximately 2-5 rem per week) there was actually a life-lengthening effect from chronic low-level radiation exposure. This, of course, applies only to *somatic* effect, and does not consider the genetic problem.

Efforts to quantitate life-shortening effects have been extrapolated from animal dosage of a larger order. The following estimates have been suggested:

(a) A man exposed to 0.3 r/week for twenty years would lose two years of life-expectancy from a sixty-five year span.

(b) Five days reduction of life-expectancy for every 1.0 r of total body dose.

(c) When exposure is 0.5 r/day, or less, reduction of life will be one day or less per roentgen of total body dose.

Well, with as many variables as there are in radiation exposure it is obvious that a formula to express life-shortening at all accurately is going to be extremely difficult to develop. It certainly is demonstrable that radiation can cause somatic damage; but by the same token we have observed somatic recovery. Who is to say that Grubbé, who lived to the age of eighty-five, had his life cut short by radiation injury, *despite* the extensive *localized* damage he had suffered?

It is interesting to review the story of permissible limits chronologically: 1928—1936, 50-100 r/year (estimated); 1936—1948, 36 r/year; 1948—1958, 15 r/year; 1958—, 12 r/year. It seems likely that the expanding use of ionizing radiation and the descending maximum permissible exposure may someday meet. When that happens, if it does, I rather think we will have devised new ways to avoid or reduce exposure. Why should we think that progress in the art of protection will stand still? Anyway, no inconvenience is involved at present. Almost all people occupationally exposed to radiation receive doses varying from 10 to 50 per cent of the maximum permitted.

Control of radio-active materials is not a new problem—we have long ago learned to take care of radium and radon with respect.

However, there may be a real question whether the astronomical expansion of the quantitative aspect in these days of nuclear radiology does or does not constitute a new kind of problem.

What is needed is a rigid system akin to surgical technique; but so much material and so many people are involved that breakdowns, thefts and accidents inevitably occur. Accidents do happen with radium: I myself once temporarily lost a radium tube down a sink drain while cleaning up. A bizarre case was reported in 1958 (9) : a scientifically curious, but rather irresponsible nineteen year old boy stole a ten milligram radium needle from a hospital, and broke it, getting himself and eventually most of his home town contaminated with the scattered salt. At several recent meetings of the national radiologic societies there have been exhibited pictures of damage to the hands of a factory worker who picked up some very "hot" radioactive sulphur inadvertently. Such accidents, and others, will have to be faced as we go into the future of nuclear radiology.

Apart from accidents, just plain routine handling, shipment and storage involve great discipline. Storage of wastes is a special headache. In an average hospital it is usually no more than a bad smelling nuisance, but on the mammoth scale of the Atomic Energy Commission's operations it involves big money and real risks. How long can we discard "hot" material into old mine shafts and ocean bottoms? This problem is, indeed, so serious that it could retard development of commercial nuclear power. One Atomic Energy Commission scientist takes an optimistic view. Citing the fact that many radio-active chemicals now useful and valuable were considered wastes a few years ago, he points out that new ideas on isotope uses are popping all the time. "The meat packers learned to use every bit of the pig except the squeal," he says; "we can do the same."

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9. *Time* Magazine: Dec. 1, 1958, p. 63.

Chapter 9

THE MODERN PRACTICE OF RADIOLOGY, ITS NON-MEDICAL ASPECTS

THIS LAST CHAPTER seemed, when I formulated the outline, to be a natural and inevitable conclusion, a summing up.

But how does one sum up an actively flowing—not to say turbulent—stream? The moment one stops to take stock, one is left behind, for the frontier is always receding ahead of us.

Actually, everyone must sum up for himself his own individual understanding of radiology's history and place in medicine. I just hope that you have found some of the material I have presented interesting enough to make you seek out the more important references.

Speaking of references, I have accumulated well over two hundred sources of all types as I dug for material. The most useful ones have been cited in each chapter. But, because the sheer volume of them discourages follow-up I think it is worth while to emphasize the following eight, which are the key ones.

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Smyth, H. D.: *Atomic Energy for Military Purposes.* Princeton University Press, 1945.

Stone, R. S.: The Concept of a Maximum Permissible Exposure. *Radiology, 58:*639, 1952.

Scott, W. G. (Ed.): *Planning Guide for Radiologic Installations.* Yearbook, Chicago, 1953.

All of these are well worth your time, but if you look up no more than one, I cannot urge you too strongly to read Otto Glasser's definitive biography of Roentgen, the second reference in the above list.

Quite apart from specific references, however, I can tell you that I have discovered a tremendous amount simply by flipping through the first forty or so volumes of *Radiology* and the *American Journal of Roentgenology*. If I were in charge of a training program, I would certainly recommend such a review to all residents during the senior year. There is a real thrill in reading the very first articles on certain subjects in the actual language of the pioneers who wrote them. This also clarifies, quite frequently, exactly what they did say, in contrast to later interpretations.

A very perspicacious radiologist named Carroll Chase (1) wrote in 1921 (when the history of radiology was a lot younger than it is today), "the study of the early literature has some value other than its purely historical interest, as it gives the student a certain perspective that it is difficult to obtain otherwise." I have felt this value of perspective strongly. Though emphatically real, it is indefinable —almost as much so as trying to explain why the study of Latin is valuable to a student of English, but no less valid.

By seeing the sort of articles written, the style, the editorial comments, and even the pictures of prominent radiologists, one can gain a little feeling of familiarity with such leaders of American radiology as Eugene Caldwell, Russell Carman, George Pfahler, Henry Pancoast, Lewis Gregory Cole, and many others. The generation before ours knew these men personally—it isn't so long ago that all the radiologists at a national convention were on a first name basis with each other as a matter of course.

What I really want to do now, I think, is to try to acquaint you with some of the practical, non-medical aspects of present day radiologic practice. These will be things not generally taught in residency, which most of us pick up only gradually, and sometimes painfully, as we go along.

I would like to stress particularly the term *non-medical.* I do not mean industrial, or agricultural, or military. I mean what was meant by one of my professors at medical school, the one teacher I remember head and shoulders above all the others, and for this reason.

One day he had a group of us, senior students, and he said, "What do you think it is that makes a young doctor get ahead?" Some of the real sharp boys came back with answers indicating that they thought depth of knowledge was the key thing. But the professor brought them up short with a thundering "*no*!" It isn't your understanding of electrolyte balance, your skill at managing diabetic acidosis, or even your cleverness in sifting through an intricate diagnostic problem. All of these are important, to be sure, and do weigh in advancement. But what he meant to emphasize was the tremendous significance of the way we *behave* to and with our colleagues, superiors and juniors. Carried to the extreme, this can verge on one-upmanship, but that is self defeating. I am talking about being a gentleman, a good businessman, and ethical. Almost all of us, with normal wholesome instincts, make a satisfactory adjustment to the professional and lay communities in which we practice, and do so naturally. But it can do no harm to notice what experiences some other doctors have had, and perhaps we can save ourselves some trouble.

Now, as it applies to radiology, I am going to discuss this in terms of the general radiologist, the man in an office or a small to moderate sized hospital. It is true that in the largest centers, it is possible for a radiologist to avoid administrative responsibility, to limit himself to diagnosis or therapy, to give up almost completely contact with patients, or to ignore isotopes entirely—or any combination of these. But it is not to such extreme specialists that I speak. Personally, I believe that the economic, administrative, and interpersonal relationship aspects of radiologic practice are of very great importance, and I may say that smooth handling of them is absolutely essential to anyone who expects to run a private office, or be the head of a radiology department of any size. Given a reasonable competence in diagnosis and therapy, I can tell you with complete conviction that the better you understand and control these non-medical functions, the better will be the quality of work your office or department does, even if your outfit doesn't become a Mecca for professional brilliance.

Before we get down to details, I want to draw attention to references number 2, 3, and 4 at the end of this chapter. They are good texts on the business and administrative sides of practice. Whether or not you care to investigate them now, you might find them useful in the future.

FINDING A POSITION

During your final year of training you will certainly begin looking about for a place to practice your specialty. It is wise to get going early, since it helps a lot to have at least the immediate future securely settled before the last few hectic months—when concern over examinations is apt to take precedence.

Some students may know exactly where they are going. But for those who do not, there are several channels of enquiry.

Personal contacts are often the best sources of ideas; people who know you personally can give the most meaningful recommendations. You have already met a number of practicing radiologists who might be able to guide you to worthwhile opportunities. These would include the chief of your own training program, and attending or consulting radiologists who come into your radiology department sufficiently often to be acquainted with you. Men who have finished training in your department one or two years ahead of you are usually excellent contacts. They would be glad to give you the benefit of their own experience.

The next thing to do is look up the placement service of the American College of Radiology. Write to the College to register. They send out periodically lists of available positions, and there is no cost involved. Placement services are also operated by many of the state or regional radiological societies, and these are worth investigating if you know pretty definitely the area you wish to live in. Hospital associations in some states often know of institutions among their own members needing a radiologist, so the secretaries of such organizations may have some good leads to offer. Commercial medical agencies are yet another avenue; you can easily locate them through the classified columns in the *Journal of the American Medical Association.*

There are, of course, quite a few opportunities, including some with industrial concerns which operate large employee health services, listed as such in the classified advertisements of the *Journal of the American Medical Association,* as well as state and regional journals. It is even possible for you yourself to announce your availability by means of a classified advertisement.

Beyond these means, there are quite a lot of positions with the government. State, county, and municipal institutions can be approached directly when you know your locality. The Army and Navy would doubtless welcome you gladly as a volunteer. The Public Health Service is some what tougher—you really have to work a bit to get in. The Veterans Administration is also a large scale program, and many of its installations offer an extremely good experience in radiology. For those who wish to make it a career, there is a definite risk of being arbitrarily transferred for the "good of the service." To others, who look on such a position as temporary, a well selected place may be very valuable in polishing one's mind for a few post-residency years, while looking about leisurely for something more definitive. Making the selection must be based on extremely critical recommendations by persons you can trust.

There are also a number of miscellaneous federal civil service jobs for physicians—the Indian Service, for instance—which might be able to place a radiologist.

Qualified radiologists are in demand. Almost any well trained, board-eligible person should have no difficulty in finding several good positions to choose from. Weigh them carefully; don't seize the first one unless it is clearly an exceptional plum and represents exactly what you want.

ORGANIZING THE OFFICE OR DEPARTMENT

Those of you who go into some sort of partnership or association, or join the staff of an institution having two or more radiologists, will as junior members, naturally conform to the existing procedures of the establishment. But, looking forward to the day when you may be independent, you should view them critically, seeing where they may be improved. Formulate your own ideas, and so be ready if ever the opportunity to develop them comes.

If you set up your own office, you will certainly need some capital. I don't know what the rock-bottom minimum is, but a friend of mine did it with $2000. He paid off the rest of the $30,000 initial investment in two years. Even so, that is quite a load of debt to work off. Anyway, real business ability is called for to manage the financing and logistics. This is an area largely unfamiliar to me, but I can heartily recommend to you Dr. J. L. McClenahan's article on the subject (5).

A very few will have the opportunity I was fortunate enough to enjoy, of organizing from zero the radiology department of a brand new hospital. It *is* possible to do this without any previous administrative experience, because I did it, but it was not easy, and I learned a lot the hard way. However, it is a very pleasant thing to build one's own organization up just the way one wants it.

A fairly common experience will be for you to assume direction of a one-man hospital department. Here you will inherit a body of procedures and relationships to the rest of the institution. You will have to change some of them, of course. Everyone has his own individual ideas of how to get things done. But one must tread carefully, and sometimes introduce changes slowly and diplomatically, so as not to alienate technicians, hospital personnel, or referring doctors.

As head of your office or department, whether it consists of you alone, or you and one or more junior men, it behooves you to know the business and administrative aspects, or at least know how to put your finger on the needed data quickly.

In the realm of business, we may include such important information as: monthly and yearly volume of work and dollar income; overhead costs; personnel salaries and raises; arrangements for dividing income between professional and overhead (hospital) categories.

Just to illustrate—I have been called on at odd times (weekends, holidays, nights, during the chief technician's or secretary's vacation) to handle such problems as these: finding records of examinations done, charges made, invoices for supplies, getting film, barium or intravenous urographic dyes when the supply unexpectedly runs out, doing something about overflowing or leaks in the dark room tanks, replacing a spoiled batch of "hypo" or developer. In all these situations, somebody has to know how to act, act fast, and act right. That somebody is the captain of the ship, *you*. You can look mighty foolish if you don't have the answers. This is why the job of captain is as important as it is. He has the responsibility, and other people can rely on him to *get things done*. Of course, he delegates most of the work, but he ought to know how to handle any breakdown anywhere.

Now, before you get stuck with the actual problem, the least you can do is prepare yourself by knowing all the procedures, by knowing where everything is, and by knowing where to find a substitute if something—anything—isn't where it should be. I do not check personally every day or week to see that my department has adequate film supplies—I can trust my chief technician to take care of that and many similar items. But if he miscalculates, or drops dead, or quits suddenly, and we run short, I jolly well know what to do about it. *Praemonitus, Praemunitus.*

You can do it differently. You may feel that these things are not professional, and decline to take administrative responsibility. In a hospital, where the service simply cannot be allowed to lapse, some one else will find a way to cope with the problems. But there are two things wrong with that. First, you lose control, and can no longer complain if anything in the administrative area doesn't suit you. Second, and most important, technical radiology is very closely bound to professional diagnosis and therapy. It has been demonstrated conclusively that it cannot be separated from them without damaging quality. Good work demands control of the technique by a doctor. If that control is to be effective, you must take the responsibility for administering it. Ideally, a workman should own his tools and take pride in them. When ownership is not feasible, he should at least have responsibility for maintaining them in the best possible condition.

Furthermore, even if you do not enjoy the administrative aspects, you might reflect that competence in them is definitely part of the non-medical virtues that get you ahead on the ladder of advancement, both academic and professional. Can you imagine being considered for an important professorship or the directorship of a large teaching hospital department if you were not a capable administrator of the place where you are?

I do not want to go into the details of procedures—these you will shape up according to your own training. You will have your own problems with such things as explaining to general practitioners why a gall bladder study should have a preliminary film—it makes an extra trip for the patient, and this generates considerable resistance. They will cite Joe Zilch, the radiologist (or pseudoradiologist) in the next town who does gall bladder-gastro-intestinal series-barium enema combination examinations all in one visit. You will have to explain—tactfully—why you do it differently.

You will certainly use techniques, procedures, record systems, and schedules that you learned to be good during your training. A good chief technician will also shape some of your plans. But, to broaden your mind, an excellent reference on all the mechanical aspects of an x-ray department is the volume edited by Dr. Wendell Scott, *Planning Guide for Radiologic Installations,* published by Yearbook in 1953 (4). This you should at least glance through before making your first move.

A word about record systems. They are so important to a radiology department that we almost take them for granted, like a good wife. But *good* record systems do not just happen, they need a lot of work. Patient, drudging work, and the secretary has to be really drilled on the need for constant vigilance and care to keep all aspects of the files up to date, avoiding errors and misfiling.

A special aspect to the records is a museum file. This requires constant attention of you, yourself, to keep it in shape, with proper additions and pruning. If well tended, it can be extremely rewarding and valuable. One feature of it that I use almost every day is my "normal book." I gradually assembled normal specimens of most bones and other parts of the body in adults, and in children at one year age intervals. I can pull a normal case of almost anything out in a minute for my own use or to demonstrate to someone else.

The value and usefulness of a museum file should be so obvious as to require no mention. But I will point out that the committee on hospital accreditation considers it an important item in mark-ing x-ray departments on their inspection. Furthermore, as you will find, one of the sports of radiologists in practice is to get together once a month or so to show each other interesting cases and discuss them. I belong to three such groups, and if I had time, there are others within reasonable travel range. Aside from being fun, and getting to see one's friends, these meetings have a very real value in cross-fertilizing our minds. We need it, because out in practice most of us do not have the teaching hospital atmosphere available. It is from our respective museum files that we pull together groups of cases for such demonstrations. In my own institution, and I am sure the same is true in many others, I hold a weekly x-ray conference for the house staff, and use the museum file regularly for this material.

RELATIONSHIPS WITH PHYSICIANS

I want to dwell a little on relationships with other doctors, non-radiologists.

Other specialists who are good in their particular branches of medicine are never a real problem. To resent their interpreting films in their fields is a pure waste of time, since you can never prevent it anyway. If, on the other hand, you cooperate in a friendly way, these orthopedists, urologists, chest physicians, neurosurgeons, etc., will almost always meet you more than half way by respecting your own competence—provided you have it—in the rest of radiology which they do *not* know, and they will consult you in the really difficult problem cases in their own areas. As a matter of fact, you can learn a very great deal from these men.

General practitioners as a rule know rather little about radiology, and you can do a lot to educate them as to how it can best serve their needs. Each one has to be treated differently, and you will gradually learn their individual peculiarities. It is important to be authoritative on radiologic matters, but overall to be gracious and helpful. Remember, these men are the sources of your patients. Even the general man who owns an x-ray machine will bring problem cases to you for interpretation if he finds you friendly.

There is one unpleasant aspect of relations with general practitioners who own x-ray machines. Every once in a while, you will run into a doctor who seems to have no scruples about examining patients in his office and then trying to obtain from you an informal, "curbstone" interpretation of the films—naturally at no cost. I know of only two ways to stop this: (a) refuse to do it, or (b) make it clear that each such consultation will be charged for.

Your reports are worth a little careful thought (6). They are one of your chief modes of contact with other doctors. Besides conveying the essential information, you will endear yourself to your referring men if you will cultivate seriously some or all of the following: clear, concise style; declarative sentences with observations and conclusions as definite and precise as possible; avoid anything vague—it just confuses the reader. Consider in any report that you can answer the doctor's diagnostic question in either the affirmative, the negative, or you don't know. If you don't know, say so point-blank and then go on to make constructive suggestions if you can: re-examination, additional views, special fluoroscopy, or other tests of value. You would do well generally to dictate a conclusion, or impression, in a separate paragraph—this is a real aid to the recipient of the report. And always make very sure that the doctor's specific diagnostic question is answered, or at least mentioned and dealt with, in the conclusion. Few things annoy a referring doctor more than to send a patient for a gastro-intestinal series with a particular question as to existence of a hiatus hernia, and then get a detailed negative report which nowhere mentions hiatus hernia.

But even beyond technical considerations of getting the data recorded in the clearest way, it is important for your reports to be in good English. I go further, and say that the best sort of report should be in elegant English, should use well chosen words and phrases which set off the important points in a way to put the findings in proper perspective, and make the reader remember them. This is just as important as wearing clean clothes and being well groomed. Do you want the fruit of your professional skill to appear crude and uncouth, or in well-bred, educated prose? It is a discourtesy to your referring man to send out a sloppy report—and I stress this because I have seen far too many of them.

Relations with other doctors around therapy cases develop a number of problems. In this area, most non-radiologists know extremely little, and the majority of them realize it. An editorial appearing in the *New England Journal of Medicine* (*265:*1166, Dec. 7, 1961) makes this point very well: "Familiarity with and a feeling of understanding and awareness of electromagnetic radiation comes only to those who constantly deal with it. It takes the average resident in radiology about six months to acquire the "feel" for x-radiation, qualitatively and quantitatively. The other practitioners of medicine never quite get to know what it is, where it comes from or how it comes about, nor what it does to living matter. Radiation is something as mysterious as outer space." Still, these men cannot allow themselves to appear too ignorant, so some rather confused ideas occasionally get passed on to patients and their families. It isn't *too* rare, for instance, to receive a post-operative patient for whom a course of several weeks treatment is in order, who has been told that all she needs is "a couple of treatments," or "a few doses of x-ray." Sometimes, but fortunately very infrequently, you may be flabbergasted to find that the patient has been told that the treatment will cost so and so much, usually a very small figure. This sort of thing has to be stopped in its tracks. You can generally train the doctors with whom you work to refer patients and leave all the therapy details to you. It takes time, and firmness, but you must make it quite clear that *you* will see the patient in consultation and *you* will decide whether or not radiotherapy is indicated. *You* will tell the patient what to expect of the treatment, how many visits it will require, and what it will cost. Only by insisting on these prerogatives can you slow down the tendency of many non-radiologists to give patients information about therapy which is misleading.

But you, on your side, have to protect the family doctor's relation to the patient, in every reasonable way conveying your respect of his competence in his own sphere, *and* sending the patient back to him after the therapy. However, in cancer cases, you often get involved in tricky footwork to keep stories told to the patient and relatives consistent, when there are usually at least three doctors involved—the family doctor, a surgeon, and yourself. In private practice you will, or should, see your therapy patient at every visit and treatment, and a rather close doctor-patient relation develops.

Maintaining a deception, when the family or other doctors concerned have initiated it, can be a very difficult thing indeed. My own experience is that when a fairly intelligent person demands flatly to know if he has cancer, you have to give him the facts. This is a big subject in its own right, of course, and I do not intend to get too far afield in *it*. All I wish to do is indicate that part of your relationship to other doctors includes a duty to support their prestige, opinions, and untruths, within reason—so long as this benefits the patient, or at least does not interfere with his proper care.

One more relationship to doctors involves availability. I think it is a poor sort of radiologist who works a certain schedule during the day and then is out of contact. The good will you can create by making it known that you are available (or that your department is covered professionally) around the clock is incalculable. I, myself, get called out at night only occasionally—but this is a negligible price to pay for the confidence of my medical community.

RELATIONSHIPS WITH TECHNICIANS

Your technicians are a very special concern of yours. Few others will accord them much credit, so you had better. If you are decent to them, they will reward you with a degree of loyalty which may surprise you. They are mostly intelligent young folk who see pretty clearly what goes on. Because they pick up a considerable smattering of practical radiology, they tend to identify with you who really know it in depth, and they feel superior to all the rest of the world who do not. You must not sneer or grudge them this pride, they don't have much else. Lord knows they are poorly enough paid. Maybe it is because amateur photography is so simple a matter, but most people seem to look down on technicians. Granted that we see some poor specimens about, most of them are pretty good. I want to quote the editor of *Radiology,* Dr. L. J. Menville, writing in 1934: "experience has proven that it requires a higher degree of intelligence, a higher degree of mechanical skill, and especially a more stable temperament successfully to meet the duties devolving upon the x-ray technician than are required of a registered nurse" (7). Well, that may be pitching it a bit strongly, but I will maintain that they are at least the equal of nurses.

The one sin of x-ray technicians which you must forestall before it happens is: interpretation of films. Most of them are well trained and will never do it. Still, the temptation is always there: late at night, a green intern seeing an emergency room case may desperately ask for help in deciding whether or not a radiograph shows a fracture. In pity, the technician will want to help him—and often could help him a lot. The point is, he mustn't. It is better to get the radiologist out of bed to decide the question.

Another risk is letting technicians set up therapy patients. I know that experienced technicians do a very good job of it in specialized therapy departments. But don't take the risk in private practice, or general hospitals. I myself once went through a nasty session because a patient complained that a technician prescribed treatment when it was only a matter of routine set-up. One avoids trouble and malpractice risk by always making every set-up himself.

RELATIONSHIPS WITH THE HOSPITAL

Hospital administrators are generally quite glad to have the radiologist run the x-ray department so long as he provides adequate service, and conforms to overall institutional policy. This includes the budget, something which has to be reviewed and agreed upon annually.

In fact, the principal areas of contact between radiologist and hospital seem to be financial. How to divide the radiology department income between the hospital (which provides the tools) and the doctor (who supplies the brains) is a vexed question which will not be completely solved for a long time. It is avoided if you have a private office, or one of the very rare rented x-ray departments, or if you accept a salaried position. But most of us have come to some sort of compromise terms. The American College of Radiology has determined that a well-run hospital radiology department should have an overhead cost which amounts to approximately sixty per cent of gross earnings (private offices, with more amenities and lower volume are apt to run higher). This means that for every $10 you charge for radiologic services it costs at least six to pay for the film, solutions, technicians, light, heat, amortization, and paper clips. It doesn't hurt to point this out to people who complain of the high price of x-rays. Very few other specialists have such a high overhead cost. The closer to 40 per cent of the gross you can make your share of the department income, the better. But hospitals do have to support their expensive non-income producing departments, and a tradition has grown up that these are paid for by the departments which do make money. Right or wrong, that is the situation, and most of us compromise at a figure lower than 40 per cent. The result, of course, is to subsidize people paying $20 or so a day for hospital beds which actually cost more than $30 to maintain and operate.

The other major inequity in the financial picture at present is the inclusion of diagnostic costs under hospitalization insurance (e.g., Blue Cross). There is some hope of correcting this eventually, since the contention that radiology is a hospital, rather than professional, service no longer commands respect in any quarter.

RELATIONSHIPS WITH LAWYERS

As soon as you get into practice, you will come face to face with the high cost of malpractice insurance. We share the highest rates with plastic surgeons and orthopedists. Of course, you cannot afford to be without it. Everyone runs the risk of being sued, or being called as a witness.

During my review of the literature I have collected a group of references dealing with malpractice litigation. I can heartily recommend these to you for instruction and entertainment (8, 9, 10, 11, 12, 13, 14). They represent a good accumulation of wisdom, and I cannot add to them.

Lawyers in general are willing to treat doctors courteously. Hostility can be minimized and cooperation promoted by liaison between county medical societies and bar associations. These contacts are well worth while, and should have your support.

RELATIONSHIPS WITH THE PUBLIC

It is surprising how few people really know what a radiologist is. Even if you say "x-ray" man, they have only a nebulous idea. But nowadays there is a tinge of alarm mixed in with the strangeness, because everyone knows that there are such things as atom bombs and radioisotopes.

For most doctors, the importance of publicity consists in preserving a favorable image of themselves and medicine generally in the public mind. Radiologists, however, have the added burden of identifying themselves as physicians. This problem has always been with us, and only a little less so now than fifty years ago. Then, it was the presence of numerous interested persons without medical degrees in the field. Now, it is the curtain between us and our patients which impedes our being understood.

Establishment of the Section on Radiology of the American Medical Association in 1925, was a major event in this struggle for recognition by organized medicine itself, and it took quite a bit of effort. Recent litigation over whether or not radiology truly is the practice of medicine shows that the issue is still dying hard.

The busier the x-ray department, the harder it becomes for us to see patients. The private office usually operates at lower volume, and the personal touch can be maintained. My own place runs at some forty to sixty diagnostic visits a day, plus perhaps ten or so more patients for therapy, radio-iodine, or follow-up visits. With two radiologists, we handle it, but cannot possibly see all the patients. The ones we do see are the fluoroscopies, the emergency room cases, the therapy patients, the isotope cases, and the follow-ups. This is all we *can* see, and still get the work done. You will find in the "Planning Guide for Radiologic Installation" (4) a figure of *23.5* as the maximum average number of cases per day that one radiologist can handle adequately.

The older generation of radiologists keeps reminding us of the importance of maintaining patient contact. I know that most of us cannot do it optimally, but we should try to do our best to keep as much contact as we can. Dr. Augustus Crane, one of the grand old men of radiology, put the case for keeping in touch with patients in very forceful terms with the following extract from his Caldwell Lecture before the American Roentgen Ray Society, in September, 1932:

"Antaeus was invincible so long as he could keep his feet on solid earth. It was only when he lost contact that Heracles was able to strangle him. The roentgenologist has his struggle with foes, economic and professional, insidious, incessant, indifferent to temporary defeat, that would have him undergo a reincarnation as a technician. But, like Antaeus, he will find himself invincible so long as his feet are planted firmly on the ground of *clinical* roentgenology."

One small problem with the public, and with other doctors too, revolves about ownership of films. This is a hangover from the early tendency to confuse radiology with photography. The issue doesn't arise very often any more, since the principle is now firmly established that ownership rests with the radiologist or hospital. The importance of this, of course, is to impress on the public that what they are paying for is a consultation and an opinion, not so many square inches of celluloid. My own state radiological society has printed a placard for display in the waiting room which makes the point succinctly, and is very useful. Naturally, one has to couple this policy with readiness to loan the films out whenever they can be of value in any consultation the patient or his family physician desires.

Besides this, we have other opportunities. I am sure I am not unique in getting several invitations every year to speak before civic and church groups, parent-teacher associations, and the like. These are all chances to put ourselves across.

There is a larger responsibility to the public, however, than acquainting it with what medical radiology is and can do. In these days when there is widespread concern over radiation hazard, we who know a little about the subject have a duty to interpret the problem to the laity and keep things in perspective (15, 16). The American College of Radiology has available a "kit" of literature on request which is very useful in preparing such talks. We also have an obligation to cooperate with civil defense planning, and should participate in training people who will have to take readings of radiation levels and decide what to do about it. Several states have undertaken at least exploratory moves in the direction of regulation of sources of ionizing radiation; whenever possible, we should cooperate and seek leadership in such planning. It is obviously important that policies in this regard be guided by sound medical and public health principles.

THE FUTURE

When I was reading over the old journals, I came across quite a lot of editorials on the general subject of "The Future of Radiology" (17, 18). Most of them were enthusiastic, or at least optimistic. However, one note of uncertainty was struck by a 1932 editorial in *Radiology* titled: "Is Roentgenology, as a Specialty, Destined to Survive?" (19). All of these articles, of course, are intensely interesting because of the light they shed on conditions at the time of writing. Nowadays we see fewer of these (20). I guess it is pretty obvious that there is a future, expanding in all directions. I just want to touch on some of the trends that are on the horizon.

I have already mentioned that the future of radio-therapy is linked with research on malignancy. Medical means of preventing or treating cancer would certainly reduce our scope. But there are still avenues to exploit, even after voltage has been raised to the limit and depth doses improved to the maximum that can be tolerated. Adjuvant physical and chemical agents—things that will potentiate the effect of ionizing radiation on malignancy—are well worth developing, and may become a major factor. Other high frequency waves—ultrasonic, at around 20 kilocycles (21), and radio, at about twenty-seven megacycles (22)—have interesting and as yet mostly unexplored effects on tissue. These may enter the therapy picture, and we should be prepared to include them in our bailiwick or else some one else will absorb them.

Regulation of radiation is, I think, coming. It will probably come slowly, since right now no one has any workable suggestions on such key matters as staffing and financing. The first thing to appear may be simple registration; actual licensure and inspection are further away. Still, the Atomic Energy Commission is making plans to carry out inspection of functions within its province. I have already stated that I cannot see any danger in regulation, assuming it is handled wisely. As a matter of fact, I believe regulation would operate to our advantage, by jacking up standards where they are lax, and weeding out borderline operators. It might finally eliminate the x-ray laboratories run by non-medical persons. Historically, these were a thorn in the side of early radiologists, but have gradually dwindled to a residual nuisance now. I can tell you it *is* an annoyance to get films through the mail from such a laboratory with an offensive rubber stamp legend all over the envelope: "Sick?—See a Chiropractor." Besides operating on that low level, these people encourage and perpetuate the idea that the films are what the patient pays for. Anyone can walk in to them, say he wants an x-ray, pay for it, and walk out with the films. Landmarks in the long campaign against them were: establishment of the Section on Radiology of the American Medical Association, and promulgation of lists of approved x-ray laboratories by the American Medical Association in 1931. This listing was superseded by the foundation of the American Board of Radiology in 1934.

A tendency which is with us now to some extent, and may increase in the future, is for radiology to fragment into subspecialties. This was foreseen and viewed editorially with alarm by such pioneers as James T. Case in 1915 and Guido Holzknecht in 1929. I have already mentioned this in connection with the division between diagnosis and therapy. Separation of isotopes from the rest of radiology is also becoming more frequent. I hear talk about the "general practitioner of radiology," as a subspecies, relegated to the sticks. Well, I won't stand in the way of progress, but perhaps because I am one of those general practitioners of radiology I rather regret the trend—if it is one. It is a commonplace thing for contemporary all-round radiologists to see persons adept in chest, urologic, orthopedic, etc., radiology show lamentable ignorance of findings in the films outside their specialty—things obvious to the radiologist because his training, even if less intense, is much more broad. Anyway, organization of medical care and volume of work being what they are at present, full time subspecialists of radiology only exist in the largest teaching centers. The general radiologist will probably be around for a long time.

What about the future of the user of x-ray apparatus who does not have what we consider proper training in radiology? One must hasten to except dentists; their use of diagnostic roentgenology is firmly established and training in this regard is adequate in American dental colleges. By contrast, few radiologists are very good at dental diagnosis. Two other fields which are borderline are dermatologic and opthalmologic radiation therapy. Most radiologists are not expert in these areas, so the specialists in those fields who have developed radiation therapy have merely filled in a vacuum.

The group I refer to is comprised of general practitioners, orthopedists, urologists, internists, and others who purchase equipment because it is legal to do so and they have the money. Competence, they usually do have, within certain bounds. But can they, will they, recognize those bounds? Well, even though most will, some won't, and I'm afraid we'll always have a few with us. Education can do something, regulation may do some more. We will be fortunate if we get to the status which surgery has reached now—when it is rather unusual for an unqualified person to practice it. At the present time, of course, they do represent a certain degree of competition to us. Most often, however, when a radiologist is available, he will get the referrals, and such operators will do only their own work. As a result, many of them will find it uneconomical. Anyway, I have yet to hear of a radiologist failing because of such competition.

And now, we have come to the end of this chapter, and of the book. I won't apologize for taking your time and attention for "non income producing" material, because I think everything we have discussed is worth your knowing about. Besides, even though it was a lot of work, it has been fun for me, and has broadened my own perspective immensely.

I want to close now with a quotation from an editorial, the very first editorial in the first number of the *American Quarterly of Roentgenology* in 1906. It is by Preston Hickey, one of the great pioneers of our specialty. It was reprinted by the *American Journal of Roentgenology, Radium Therapy and Nuclear Medicine* in its anniversary volume of 1956, to remind us of how conditions were in the days when medical radiology was getting to its feet.

"Roentgenology is a department of medical science that has grown to such an extent that its followers are entitled to a certain respect for their work and their conclusions. This respect is not always accorded. The reasons for this are not hard to find. Many of the workers in this field have been content to be classed as photographers and picture makers, allowing their plates to pass through other hands and to be interpreted by others. They have been careless in the choice of terms to be employed. They have allowed the term "Roentgenogram" or "diagnostic plate" to be supplanted by the terms "photography," "picture," etc. This carelessness on their own part cannot but lead to a similar carelessness on the part of those with whom they come in contact. It is time that the experts in the science of Roentgenology demand for their work the recognition which is due them. To receive this respect, it is necessary for the Roentgenologist to preserve a proper dignity in referring to his diagnostic work. He should be careful in his nomenclature, preferring such terms as Roentgenogram, diagnostic plate, etc. He should, in general, let the profession and laity understand that the fee for his services is based upon the diagnostic conclusions which he reaches. He should maintain in his own hands his Roentgenogram, delivering simply the diagnosis. He should avoid arguing with the laity concerning interpretations of his records of density. In other words, he should maintain always the position of consultant, who gives diagnostic conclusions based upon special lines of investigation. In regard to treatment, he should not allow himself to be dictated to, either as to the number or character of his treatments by the attending physician. He should give the profession and the patient to understand that all the details of Roentgen therapeutics are guided by his judgement and experience.

It would seem that if Roentgenologists would adopt an attitude and position such as above outlined, they would receive the respect which is their proper due."

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